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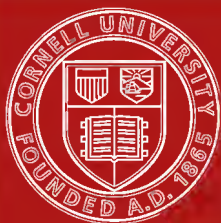
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Various fragments, by Herbert Spencer.



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P R E F A C E.

Several of the following fragments contain ideas and suggestions which ought not, I think, to remain buried, and practically lost, in their original places of publication. Preservation may, I think, prove to be of some importance. As for the rest, I do not know that they are all intrinsically of such value as to be worthy of a permanent form. But it has seemed that along with republication of the fragments of chief significance, there might fitly go a republication of those of less significance, which would not have been worth republishing by themselves.

H. S.

July, 1897.

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THE BOOKSELLING-QUESTION.

In April 1852 was published in the *Westminster Review*, an essay on "The Commerce of Literature," written by Mr. (afterwards Dr.) Chapman, then a publisher and the owner of the review. The picture which it drew of the trade-regulations and their results, initiated an agitation among authors, in which I took part, and in furtherance of it published the following letter in *The Times* for April 5, 1852. This letter, signed "An Author." I reproduce partly because it shows the condition of the book-trade in the middle of the century, and partly because it bears significantly on a current question — namely whether the system then in force shall be re-established.

Somewhat more than a year since I published a work of which the advertised price is 12s. I have now before me an account up to Christmas last, wherein I find myself credited with the copies sold at the rate of 8s. 6d. each. The trade custom of giving 25 for the price of 24 reduces this to somewhat less than 8s. 2d.. Further, my publisher deducts 10 per cent. commission for all sales he makes in my behalf; so that ultimately the net sum per copy payable to me becomes 7s. 4d. Out of this 7s. 4d.

per copy I have to pay for the composition, printing, paper, and binding; for the advertising, which threatens to reach 50*l.*; and for the 30 odd copies sent to the national libraries, newspapers, and reviews. The result is that, though of its kind the book has been a very successful one, my account up to Christmas last shows a balance of 80*l.* against me. Possibly in 18 months hence the work will have paid its expenses, and I am even not without hope that it will leave me some 10*l.* in pocket as a reward for my two years' toil. Should it do so, however, I shall be unusually fortunate; for my publisher tells me that the great majority of works having, like mine, a philosophical character, entail loss.

Now, with all their skill in mystification, the Booksellers' Association will find it difficult to show that out of a selling price of 12*s.* the proportion set aside to pay for printing, paper, binding, advertising, gratuitous copies, and author, should be 7*s.* 4*d.*, while 4*s.* 8*d.* may reasonably be charged for conveyance to the reader. In these days of cheap carriage 60 per cent. for cost of production, and 40 per cent. for portorage, is a somewhat anomalous division.

Mr. Murray says it is in great measure an author's question. He is right, and authors will prove much less intelligent than I take them to be if they do not see how immensely their own interests, as well as those of the public, would be served by a diminution of these exorbitant trade profits. Let any one refer to *Porter's Progress of the Nation*, and there note the many cases in which a small reduction of price has been followed by a great increase of consumption, and he cannot avoid the inference that a 20 per cent. decrease in the vendor's charge for a book would cause a much more than proportionate increase in its sale; and as this decrease would be in the cost of agency, and not in the author's price, the extra sale would be so much clear profit to him. Books that now entail

loss would pay their expenses, and books that now only pay their expenses would bring something like a reasonable remuneration.

Should the publishers and booksellers persist in their restrictive policy, which is injurious not only to authors and the public, but, I believe, in the long run even to themselves, I think that as a matter of business authors will be justified in declining to publish with any who belong to the combination.

The movement initiated, as above said, by Mr. Chapman's article, resulted in an agreement to arbitrate between the authors and the traders. The arbitrators appointed were Lord Campbell, Mr. Grote, and Dean Milman. They gave their decision in favour of the authors, and the trade-regulations which enforced the system of "net prices" were at once abolished.

AN ELEMENT IN METHOD.

The fragment which here follows was originally the introductory chapter to Part III of *The Principles of Psychology* (first edition 1855). When preparing a second and enlarged edition of the work in 1868—70, I omitted it as not being relevant to Psychology in particular but as being relevant rather to science in general; and I then entertained the thought of making it part of an essay "On Method" to be prefixed to *First Principles*. Pre-

occupation prevented me from carrying out that intention and ill-health now obliges me to abandon it. But the thought which this fragment embodies has, I think, a degree of importance which makes preservation desirable; and I therefore decide to include it in this volume.

It is a dominant characteristic of Intelligence, viewed in its successive stages of evolution, that its processes, which, as originally performed, were not accompanied with a consciousness of the manner in which they were performed, or of their adaptation to the ends achieved, become eventually both conscious and systematic. Not simply is this seen on comparing the actions popularly distinguished as instinctive and rational; but it is seen on comparing the successive phases of rationality itself. Thus, children reason, but do not know it. Youths know empirically what reason is, and when they are reasoning. Cultivated adults reason intentionally, with a view to certain results. The more advanced of such presently inquire after what manner they reason. And finally, a few reach a state in which they consciously conform their reasonings to those logical principles which analysis discloses. To exhibit this law of mental progress clearly, and to show the extent of its application, some illustrations must be cited.

Classification supplies us with one. All intelligent action presupposes a grouping together of things possessing like properties. To know what is eatable and what not; which creatures to pursue and which to fly; what materials are fit for these purposes and what for those; alike imply the arrangement of objects into classes of such nature that, from certain sensible characteristics of each, certain other characteristics are foreseen. It is manifest that throughout all life, brute and human, more or less of this discrimination is exercised; that it is more exer-

cised by higher creatures than by lower; and that successful action is in part dependent on the extent to which it is pushed. Now it needs but to open a work on Chemistry, Mineralogy, Botany, or Zoology, to see how this classification which the child, the savage, and the peasant, carry on spontaneously, and without thinking what they are doing, is carried on by men of science systematically, knowingly, and with deliberate purpose. It needs but to watch their respective proceedings, to see that the degrees of likeness and unlikeness, which unconsciously guide the ignorant in forming classes and subclasses, are consciously used by the cultured to the same end. And it needs but to contrast the less advanced men of science with the more advanced, to see that this process of making groups, which the first pursue with but little perception of its ultimate use, is pursued by the last with clear ideas of its value as a means of achieving higher objects.

So too is it with nomenclatures. Few will hesitate to admit that in the first stages of language, things were named incidentally—not from a recognition of the value of names as facilitating communication; but under the pressure of particular ideas which it was desired to convey. The poverty of aboriginal tongues, which contain words only for the commonest and most conspicuous objects, serves of itself to show, that systems of verbal signs were, in the beginning, unconsciously extended as far only as necessity impelled. Now, however, nomenclatures are made intentionally. A new star, a new island, a new mineral, a new plant or animal, are severally named by their discoverers as soon as found; and are so named with more or less comprehension of the purpose which names subserve. Moreover it may be remarked that whereas, in the primitive unconscious process of naming, the symbols employed were, as far as might be, descriptive of the things signified; so, in our artificial systems of names—and especially in our chemical one—a descript-

ive character has been designedly given. Add to which, that whereas there spontaneously grew up in natural nomenclatures, certain habitual ways of combining and inflecting names to indicate composite and modified objects; so, in the nomenclatures of science, systematic modes of forming compound names have been consciously adopted.

Again, a similar progress may be traced in the making of inductions. As is now commonly acknowledged, all general truths are either immediately or mediately inductive—are either themselves derived from aggregations of observed facts, or are deduced from truths that are so derived. The grouping together of the like coexistences and sequences presented by experience, and the formation of a belief that future coexistences and sequences will resemble past ones, is the common type of all initial inferences, whether they be those of the infant or the philosopher. Up to the time of the Greeks, mankind had pursued this process of forming conclusions, unknowingly, as the mass of them pursue it still. Aristotle recognized the fact that certain classes of conclusions were thus formed; and to some extent taught the necessity of so forming them. But it was not until Bacon lived, that the generalization of experiences was erected into a method. Now, however, that all educated men are in a sense Bacon's disciples, we may daily see followed out systematically, and with design, in the investigations of science, those same mental operations which mankind at large have all along unwittingly gone through, in gaining their commonest knowledge of surrounding things. And further, in the valuable "System of Logic" of John Mill, we have now exhibited to us in an organized form, those more complex intellectual procedures which acute thinkers have ever employed, to some extent, in verifying the aboriginal inductive process—procedures which the most advanced inquirers are now beginning to employ

with premeditation, and with a recognition of their nature and their purpose.

Another illustration may be drawn from the first part of this work. On reconsidering the chapter treating of the Universal Postulate, it will be seen that the canon of belief there enunciated as the one to be used in testing every premiss, every step in an argument, every conclusion, is one which men have from the beginning used to these ends; that beliefs which are proved by the inconceivableness of their negations to invariably exist, men have, of necessity, always held to be true, though they have not knowingly done this; and that the step remaining to be taken, was simply to apply this test consciously and systematically. It will also be seen that the like may be said of the second canon of belief contained in that chapter; viz. that the certainty of any conclusion is great, in proportion as the assumptions of the Universal Postulate made in reaching it are few. For as was pointed out people in general habitually show but little confidence in results reached by elaborate calculations, or by long chains of reasoning; whilst they habitually show the greatest confidence in results reached by direct perception; and these contrasted classes of results are those which respectively presuppose very many and very few assumptions of the Universal Postulate. In this case therefore, as in the other, the rational criterion is simply the popular criterion analyzed, systematized, and applied with premeditation.

In further exemplification of this law I might enlarge upon the fact, that having found habit to generate facility, we intentionally habituate ourselves to those acts in which facility is desired; upon the fact, that having seen how the mind masters its problems by proceeding from the simple to the complex, we now consciously pursue our scientific inquiries in the same order; upon the fact, that having, in our social operations, spontaneously fal-

len into division of labour, we now, in any new undertaking, introduce division of labour intentionally. But without multiplying illustrations, it will by this time be sufficiently clear, that, as above said, not only between the so-called instinctive processes and rational ones, is there a difference in respect of the consciousness with which they are performed, but there are analogous differences between the successive gradations of rationality itself.

Are we not here then, led to a general doctrine of methods? In each of the cases cited, we see an arranged course of action deliberately pursued with a view to special ends—a method; and on inquiring how one of these methods differs from any conscious intelligent procedure not dignified by the title, we find that it differs only in length and complication. Neglecting this distinction as a merely conventional one—ceasing to regard methods objectively, as written down in books, and regarding them subjectively, as elaborate modes of operation by which the mind reaches certain results—we shall see, that they may properly be considered as the highest self-conscious manifestations of the rational faculty. And if, viewed analytically, all methods are simply complex intellectual processes, standing towards conscious reasoning much as conscious reasoning stands towards unconscious reasoning, and as unconscious reasoning stands towards processes lower in the scale—if further, in the several instances above given, methods arose by the systematization and deliberate carrying out of mental operations which were before irregularly and unwittingly pursued—may we not fairly infer that all methods arise after this manner? That they become methods, when the processes they embody have been so frequently repeated as to assume an organized form? And that it is the frequent repetition, which serves alike to give them definiteness, and to attract consciousness to them as processes by which certain ends

have been achieved. Is it not indeed obvious, *à priori*, that no method can be practicable to the intellect save one which harmonizes with its pre-established modes of action? Is it not obvious that the conception of a method by its promulgator implies in the experiences of his own mind, cases in which he has successfully followed such method? Is it not obvious that the advance he makes, consists in observing the processes through which his mind passed on those occasions, and generalizing and arranging them into a system? And is it not then obvious that, both in respect of origin and applicability, no method is possible but such as consists of an orderly and habitual use of the procedures which the intellect spontaneously pursues, but pursues fitfully, incompletely, and unconsciously? The answers can scarcely be doubtful.

By thus carrying consciousness a stage higher, and recognizing the method by which methods are evolved, we may perhaps see our way to further devices in aid of scientific inquiry. As in the case of deductive logic, and classification, and nomenclature, and induction, and the rest, it happened that by becoming conscious of the mode in which the mind wrought in these directions, men were enabled to organize its workings, and consequently to reach results previously unattainable; so, it is possible that by becoming conscious of the method by which methods are formed, we may be assisted in our search after further methods. If in the instances given, the method of forming methods was that of observing the operations by which from time to time the mind spontaneously achieved its ends, and arranging these into a general scheme of action to be constantly followed in analogous cases; then, in whatever directions our modes of inquiry are at present unmethodized, our policy must be to trace the steps by which success is occasionally achieved in these directions; in the hope that by so doing, we may be enabled to frame systems of procedure

which shall render future successes more or less sure. That there is scope for this cannot be doubted. On remembering how much, even of the best thinking, is done in an irregular way; how little of the whole chain of thought by which a discovery is made, is included in the bare logical processes; and how unorganized is the part not so included; it will be manifest that there are intellectual operations still remaining to be methodized. And here may fitly be introduced an example, to which, in fact, the foregoing considerations are in a manner introductory.

Every generalization is at first an hypothesis. In seeking out the law of any class of phenomena, it is needful to make assumptions respecting it, and then to gather evidence to prove the truth or untruth of the assumptions. The most rigorous adherent of the inductive method, cannot dispense with such assumptions; seeing that without them, he can neither know what facts to look for, nor how to interrogate such facts as he may have. Hypotheses, then, being the indispensable stepping-stones to generalizations—every generalization having to pass through the hypothetic stage—it becomes a question whether there exists any mode of guiding ourselves towards true hypotheses. At present, hypotheses are chosen unsystematically—are suggested by cursory inspections of the phenomena; and the seizing of right ones, seems, in the great majority of cases, a matter of accident. May we not infer however, from the peculiar skill which some men have displayed in the selection of true hypotheses, that there is a special kind of intellectual action by which they are distinguishable. To call the faculty shown by such men, genius, or intuition, is merely to elude the question. If mental phenomena conform to fixed laws, then, an unusual skill in choosing true hypotheses, means nothing else than an unusual tendency to pursue that mental process by which true hypotheses are

reached; and this implies that such a process exists.

To identify this process is the problem: to find how, when seeking the law of any group of phenomena, we may make a probable assumption respecting them—how we may guide ourselves to a point of view from which the facts to be generalized can be seen in their fundamental relations. Evidently, as the thing wanted is always an unknown thing, the only possible guidance must be that arising from a foreknowledge of whereabouts it is to be found, or of its general aspect, or of both. If all true generalizations (excluding the merely empirical ones) should possess a peculiarity in common; and this peculiarity should be one not difficult of recognition; the desired guidance may be had. That such a peculiarity exists, will by this time have been inferred; and it now remains to inquire what it is.

Most are familiar with the observation, that viewed in one of its chief aspects, scientific progress is constantly towards larger and larger generalizations—towards generalizations, that is, which include the generalizations previously established. Further, the remark has been made, that every true generalization commonly affords an explanation of some other series of facts than the series out of the investigation of which it originated. In both of which propositions we have partial statements of the truth, that each onward step in science is achieved when a group of phenomena to be generalized is brought under the same generalization with some connate group previously considered separate. Let us look at a few cases.

In the Calculus it was thus, when the relationships of extension, linear, superficial, and solid, were found to conform to the same law with those of numbers that are multiplied into each other; and again, when numbers themselves, whether representing spaces, forces, times, objects, or what not, were found to possess certain general properties, capable of being expressed algebraically,

which remain the same whatever the magnitudes of the numbers. In Mechanics it was thus, when a formula was discovered which brought the equilibrium of the scales, under the same generalization with the equilibrium of the lever with unequal arms: and again, when the discovery that fluids press equally in all directions, afforded explanations, alike of their uniform tendency towards horizontality, and of their power to support floating bodies. Thus too was it in Astronomy, when the apparently erratic movements of the planets, and the comparatively regular movement of the moon, were explained as both due to similar revolutions; and when the celestial motions, and the falling of rain-drops, were explained as different manifestations of the same force. It was thus in Optics, when the composite nature of light was discovered to be the passive cause of the prismatic spectrum, of the rainbow, and of the colours of objects; in Thermotics, when the expansion of mercury, the rising of smoke, and the boiling of water, were recognized as different manifestations of the same law of expansion by heat; in Acoustics, when the doctrine of undulations was found to apply equally to the phenomena of harmonies, of discords, of pulses, of sympathetic vibrations. Similarly, it was thus in Chemistry, when the burning of coal, the rusting of iron, and the wasting away of starved animals, were generalized as instances of oxidation. It was thus too, when the electro-positive and electro-negative relations of the elements, were brought in elucidation of their chemical affinities. And once more it was thus, when, by the investigations of Oersted and Ampère, the phenomena of Electricity and Magnetism were reduced to the same category; and the behaviour of the magnetic needle was assimilated to that of a needle subjected to the influence of artificial electric currents.

Now this circumstance, that a true generalization

usually brings within one formula groups of phenomena which at first sight seem unallied, is itself a more or less reliable index of the truth of a generalization. For manifestly, to have found for any series of facts, a law which equally applies to some apparently distinct series, implies that we have laid hold of a truth more general than the truths presented by either series regarded separately—more general than the truths which give the special character to either series. If, in the instances above cited, and in hosts of others, we find that the most general fact displayed by any class of phenomena, is also the most general fact displayed by another class, or by several other classes; then, we may conversely infer, on finding a general fact to be true of several cases in each of two separate classes, that there is considerable probability of its being true of all the cases in each class. Or, to exhibit the proposition in another form:—A peculiarity observed to be common to cases that are widely distinct, is more likely to be a fundamental peculiarity, than one which is observed to be common to cases that are nearly related.

Hence, then, is deducible a method of guiding ourselves towards true hypotheses. For if a characteristic seen equally in instances usually placed in different categories, is more likely to be a general characteristic than one seen equally in instances belonging to the same category; then, it is obviously our policy, when seeking the most general characteristic of any category, not to compare the instances contained in it with each other, but to compare them with instances contained in some allied category. We must seek out all the categories with which alliance is probable; compare some of the phenomena included in each with some of the phenomena under investigation; ascertain by each comparison what there is common to both kinds; and then, if there be any characteristic common to both, inquire whether it

is common to all the phenomena we are aiming to generalize: in doing which we may with advantage still act out the same principle, by comparing first the cases that are most strongly contrasted. The adoption of this course secures two advantages. Not only must any peculiarity which may be hit upon, as common to phenomena of separate classes, have a greater probability of being a generic peculiarity, than any one of the many peculiarities possessed in common by phenomena of the same classes; but further, we shall be more likely to observe *all* that there is in common between diverse phenomena placed side by side, than we shall to observe *all* that there is in common between phenomena so much alike as to be classed together. Fewer hypotheses are possible; all that are possible are likely to be thought of; and of those thought of, each has a much higher chance of being true.

PROFESSOR CAIRNES'S CRITICISMS.

Professor Cairnes having, in *The Fortnightly Review* for January and February 1875, criticized my views concerning social evolution and its relations to individual volitions and activities, I published in the February number the following reply, which, by the Editor's courtesy, I was allowed to append to Prof. Cairnes's concluding article. The prevalence of the error into which Prof. Cairnes fell, makes desirable the reproduction of this explanation excluding it.

Were it possible to expound clearly, in one small volume, a doctrine which three large volumes are to be

occupied in expounding, it would be needless to write the three large volumes. Further, in a work on the *study* of a science devoted to the discussion of difficulties & preparations, and referring to its facts and inferences mainly in elucidation of the study, it is hardly to be expected that the *principles* of the science can be set forth with the exactness and the qualifications proper to a work on the science itself; indications and outline statements only are to be looked for.

I say this by way of implying that the objections raised by Professor Cairnes to views incidentally sketched in the *Study of Sociology*, will be adequately met by the full exposition which the *Principles of Sociology* is to contain. This exposition will, I believe, satisfy Professor Cairnes that he does not quite rightly apprehend the general doctrine of evolution, and the doctrine of social evolution forming part of it. For example, so far is it from being true, as he supposes, that the existence of stationary societies is at variance with the doctrine, it is, contrariwise, a part of the doctrine that a stationary state, earlier or later reached, is one towards which all evolutionary changes, social or other, inevitably lead. (See *First Principles*, chap. XXII, "Equilibration."). And again, so far is it from being true that the slow social decays which in some cases take place, and the dissolutions which take place in others, are incongruous with the doctrine, it is, contrariwise, a part of it that decays and dissolutions must come in all cases. (See *First Principles* chap. XXIII., "Dissolution")

Leaving the rest of Professor Cairnes's objections to be answered by implication in the volumes which I hope in time to complete, I will here say no more than may suffice to remove the impression that I advocate passivity in public affairs. From the principles laid down, he considers me bound to accept the absurd corollary that political organization is superfluous. To recall his illu-

stration of insurance against fire, he argues that since loss by fire is not diminished by insurance companies, but only re-distributed, I must, in pursuance of my argument, hold that insurance companies are useless! The passage which Professor Cairnes quotes is directed against "the current illusion that social evils admit of *radical* cures," in immediate ways; and insists "that *the question in any case* is whether re-distribution, even if practicable, is desirable:" the obvious implication being that some re-distributions are desirable and some not.

I am chiefly concerned, however, to repudiate the conclusion that "the private action of citizens" is needless or unimportant, because the course of social evolution is determined by the natures of citizens, as working under the conditions in which they are placed. To assert that each social change is thus determined, is to assert that all the egoistic and altruistic activities of citizens are factors of the change; and is tacitly to assert that in the absence of any of these — say political aspirations, or the promptings of philanthropy — the change will not be the same. So far from implying that the efforts of each man to achieve that which he thinks best, are unimportant, the doctrine implies that such efforts, severally resulting from the natures of the individuals, are indispensable forces. The correlative duty is thus emphasized in § 34 of *First Principles*: —

"It is not for nothing that he has in him these sympathies with some principles and repugnance to others. He, with all his capacities, and aspirations, and beliefs, is not an accident, but a product of the time. He must remember that while he is a descendant of the past, he is a parent of the future; and that his thoughts are as children born to him, which he may not carelessly let die. He, like every other man, may properly consider himself as one of the myriad agencies through whom works the Unknown Cause; and when the Unknown Cause produces in him a certain belief, he is thereby author-

ized to profess and act out that belief. For, to render in their highest sense the words of the poet, —

'...Nature is made better by no mean,
But nature makes that mean: over that art
Which you say adds to nature, is an art
That nature makes.'

That there is no retreating from this view in the work Professor Cairnes criticizes, is sufficiently shown by its closing paragraph: —

"Thus, admitting that for the fanatic some wild anticipation is needful as a stimulus, and recognizing the usefulness of his delusion as adapted to his particular nature and his particular function, the man of higher type must be content with greatly-moderated expectations, while he perseveres with undiminished efforts. He has to see how comparatively little can be done, and yet to find it worth while to do that little: so uniting philanthropic energy with philosophic calm."

I do not see how Professor Cairnes reconciles with such passages, his statement that "according to Mr. Spencer, the future of the human race may be safely trusted to the action of motives of a private and personal kind — to motives such as operate in the production and distribution of wealth, or in the development of language." This statement is to the effect that I ignore the "action of motives" of a higher kind; whereas these are not only necessarily included by me in the totality of motives, but repeatedly insisted upon as all-essential factors. I am the more surprised at this misapprehension, because, in the essay on "Specialized Administration", to which Professor Cairnes refers (see *Fortnightly Review*, for December, 1871), I have dwelt at considerable length on the altruistic sentiments and the resulting social activities, as not having been duly taken into account by Professor Huxley.

As Professor Cairnes indicates at the close of his first paper, the difficulty lies in recognizing human actions

as, under one aspect, voluntary, and under another pre-determined. I have said elsewhere all I have to say on this point. Here I wish only to point out that the conclusion he draws from my premises is utterly different from the conclusion I draw. Entering this caveat, I must leave all further elucidations to come in due course.

VIEWS CONCERNING COPYRIGHT.

In 1877 a Royal Commission sat to take evidence on the general question of Copyright, and I was invited to give evidence. The following result is reproduced from the official "Minutes of Evidence" given on March 6 and March 20, 1877. On a subsequent occasion (May, 1881) at a meeting of the National Association for the Promotion of Social Science, I expressed some further views, which were published in vol. XIV of the society's *Proceedings*.

(*Chairman.*) I need hardly ask, you are a writer of philosophical and scientific books?—I am.

Would you give the Commission your experience of the terms on which you published your first book?—I published my first work, "Social Statics," at the end of 1850. Being a philosophical book it was not possible to obtain a publisher who would undertake any responsibility, and I published it at my own cost. A publisher looks askance at philosophy, and especially the philosophy of a new man; hence I published on commission.

Would you like to state what the result was?—The edition was 750; it took 14 years to sell.

Then with respect to your next work?—In 1855 I

published the "Principles of Psychology"; I again tried in vain to get a publisher, and published again at my own cost. There were 750 copies, and the sale was very slow. I gave away a considerable number, and the remainder, I suppose about 650, sold in $12\frac{1}{2}$ years.

Have you had any other similar cases? Yes; I afterwards, in 1857, published a series of Essays, and, warned by past results, I printed only 500. That took $10\frac{1}{2}$ years to sell. After that a second series of Essays, and a little work on Education, which both had kindred results, but were not quite so long in selling. I should add that all these sales would have taken still longer but for the effect produced upon them by books published at a later period, which helped the earlier ones to sell.

Have all these subsequent works to which you now refer been published in the same way?—No. Towards 1860 I began to be anxious to publish a "System of Philosophy," which I had been elaborating for a good many years. I found myself in the position of losing by all my books; and after considering various plans, I decided upon the plan of issuing to subscribers in quarterly parts, and to the public in volumes when completed. Before the initial volume, "First Principles," was finished, I found myself still losing. During issue of the second volume, the "Principles of Biology," I was still losing. In the middle of the third volume I was still losing so much, that I found I was frittering away all I possessed. I went back upon my accounts, and found that in the course of 15 years I had lost nearly 1,200*l.*—adding interest, more than 1,200*l.*; and as I was evidently going on ruining myself, I issued to the subscribers a notice of cessation.

Was that loss the difference between the money that you had actually spent in publishing the books and the money you had received in return?—Not exactly. The difference was between my total expenditure in publish-

ing the books and living in the most economical way possible, and the total returns. That is to say, cutting down my expenses to the smallest amount, I lost 1,200*l.* by the inadequate returns, and trenched to that extent upon capital.

But you continued afterwards, did you not, to publish?—I continued afterwards, simply, I may say, by accident. On two previous occasions, in the course of these 15 years, I had been enabled to persevere, spite of losses, by bequests. On this third occasion, after the issue of the notice, property which I inherit came to me in time to prevent the cessation.

May I ask how long it took before you began to be repaid for your losses?—My losses did not continue very long after that: the tide turned and my books began to pay. I have calculated what length of time it has taken to repay my losses, and find they were repaid in 1874; that is to say, in 24 years after I began I retrieved my position.

Then the Commission understand that your books are now remunerative?—They are now remunerative, and for this reason:—As I have explained, I had to publish on commission. Commission is a system which, throwing all the cost upon the author, is very disastrous for him if his books do not pay, and, as you see in this case, has been very disastrous to me; but when they do pay it is extremely advantageous, inasmuch as in that case the publisher who does the business takes only 10 per cent, and the whole of the difference between cost and proceeds, minus that 10 per cent, comes to the author. I have calculated what are my actual returns, on two suppositions. I have ascertained the percentage I get upon 1,000 copies, supposing that I set up the type solely for that 1,000 copies—supposing, that is, that the cost of composition comes into the cost. In that case I reap $30\frac{3}{4}$ per cent. But I reap much more. I was sanguine enough when I

began this series of books, to stereotype. The result is that now I simply have to print additional thousands as they are demanded. If I suppose the cost of composition and stereotyping to have been paid for the first edition, and only estimate the cost of paper and printing in the successive editions, then I am reaping $41\frac{3}{4}$ per cent. The actual percentage, of course, is one which lies between those two; but year by year, with each additional thousand, I approach more nearly to the limit of $41\frac{3}{4}$ per cent. I should point out that the result of this is that I receive, as may be supposed, a considerable return upon the moderate numbers sold.

And that being so, can you tell the Commission what in your opinion would have happened had there been in existence a system under which three years, say, after date of publication anyone could have reprinted your books, paying you a royalty of 10 per cent:—The result would have been that my losses would not have been repaid now. After 26 years work I should still have been out of pocket; and should be out of pocket for many years to come.

(*Mr. Trollope.*) Under such a system do you think that you would ever have recovered that money?—I am taking it on the most favourable supposition, merely supposing that all other things but the percentage had remained the same.

(*Chairman.*) Assuming the system of royalty to be in existence, what would be the result on your present returns, supposing losses to have been repaid?—Between two thirds and three fourths of those returns would be cut off. They would be reduced to little more than a fourth of their present amount.

(*Sir H. Holland.*) How do you arrive at that result?—By comparing the supposed percentage with the percentage I actually receive.

Assuming a royalty of 10 per cent. upon the retail

price?—Yes.

(*Chairman.*) Would it not be probable that the reduction in price of your books would so increase the sales that you would reap a larger return than you have supposed in the estimate that you have now given?—I think not, or very little. First of all for the reason that the amount of reduction would not be anything like so great as at first sight appears. If a publisher issued rival editions of my books without my assent, on paying a royalty, he would only do so to make a profit beyond that which mere commission would bring. My present publisher is content with 10 per cent. commission. A publisher who competed as a speculation would want to make his profit beyond the 10 per cent. commission: as I ascertain, probably, at least a further 10 per cent. Then there would be my own 10 per cent. royalty. So that I find the reduction in price under such a royalty system would only be about 15 per cent. That is to say, the reduction would be from 20s. to 17s. Now I am of opinion that a reduction of the price of one of my books by that amount would have but a small effect upon the sales, the market being so limited. Let me use an illustration. Take such a commodity as cod-liver oil, which is a very necessary thing for a certain limited class. Suppose it is contended that, out of regard for those to whom it is so necessary, retailers should be compelled to take a smaller profit, and you reduce the price by 15 per cent. The consumption would be very little influenced, because there would be none except those who had it prescribed for them who would be willing to take it, and they must have it. Now take one of my books, say the “Principles of Psychology.” Instead of calling it “*caviare* to the general,” let us call it cod-liver oil to the general: I think it probable that if you were to ask 99 people out of 100 whether they would daily take a spoonful of cod-liver oil or read a chapter of that book, they would prefer the cod-liver oil.

And if so it is quite clear, I think, that no lowering of the price by 3*s.* out of 20*s.* would in any considerable degree increase the number of persons who bought the "Principles of Psychology." The class is so limited and so special that there would be no increase of profit of a considerable kind in consequence of an increased number sold.

(*Mr. Trollope.*) But are there not many people who would have benefited by cod-liver oil who cannot get it at present because of the price?—I think in all those cases in which they would be benefited they get it by hook or by crook when it is prescribed for them.

And in the same way with your books you think?—Yes. For instance, university men have to read them, and they would buy them in any case.

(*Chairman.*) What would have happened to you originally had there been a law giving a copyright only of short duration, under such an arrangement of percentage as that which you have just named?—I think it is tolerably obvious, from what I have already said, that I should not have been wholly deterred. I should have gone on losing for many years; but I think it is also clear that I should have stopped short much sooner than I did. Every author is naturally sanguine about his books: he has hopes which nobody else entertains. The result is that he will persevere, in the hope of at some time or other reaping some return, when to other persons there seems to be no probability of the kind. But supposing it becomes manifest to him that the copyright law is such that when his books succeed, if they ever do succeed, he will not get large profits, then the discouragement will be much greater, and he will stop much sooner. If I, for instance, instead of seeing that under the system of commission I should eventually, if I succeeded, repay myself and get a good return, had seen that eventually, if I succeeded, I should receive but small gains, I should

have given it up.

Are there other publications which you have undertaken besides those to which you have already referred?—Yes. About 10 years ago I commenced preparing works now published under the name of “Descriptive Sociology,” in large folio parts, and containing tables and classified extracts representing the civilisations of various societies. I employed gentlemen to make these compilations.

Do you wish to state what has been the result of that undertaking so far?—Yes. I made up my accounts last Christmas. I had then in the course of those 10 years expended 2,958*l.* odd upon eight parts (five published and three in hand), and my nett return from sales of the five parts published in England and America was 608*l.* 10*s.*

May I ask whether you ever expect to get back the money you have expended?—I may possibly get back the printing expenses on the earliest part, and most popular part, that dealing with the English civilization, in 1880, at the present rate of sale. The printing expenses of the other parts I do not expect to get back for many years longer. The cost of compilation I expect to get back if I live to be over 100.

(*Mr. Daldy.*) You spoke of the circulation in England and America. May I ask, Do you send stereotype plates to America?—I did at first send stereotype plates to America, but the thing having proved to be so great a loss I now send a portion of the printed edition.

(*Chairman.*) May I ask why do you expect repayment of the cost of compilation to be so slow as you stated in your answer to my last question?—The reason is that I made a promise to the compilers entailing that. The compilers are university men, to whom I could afford to give only such salaries as sufficed for their necessary expenses. To make the thing better for them, and to be

some incentive, I told them, that when the printing expenses on any one part were repaid, I would commence to divide with the compiler of it the returns of subsequent sales: the result being, that the cost of compilation comes back to me only at half the previous rate. I name this because it shows that in the absence of a long copyright, I could have given no such contingent advantage to the compilers. I wish to point out another way in which a short copyright would have impeded me. As a further incentive to these compilers to do their work well, as also make the prospect better for them, I gave them to understand that the copyrights and the stereotype plates would be theirs after my death. Of course with a short copyright I could not have done that.

Then in your opinion it is only by a long duration of copyright that you can be enabled to recover any considerable part of the money that you have sunk in these publications?—Certainly. If it were possible for anyone to reprint, such small return as goes towards diminishing this immense loss would be in part intercepted.

But if this work, which you call “Descriptive Sociology”, is so unremunerative, how do you imagine you would be in danger of having it reprinted under the suggested system of royalty?—It appears at first sight not a rational expectation, but it is perfectly possible. Each number of the work consists of a set of tables and a set of classified extracts. It was suggested by a reviewer of the first part, the English part, that the tables should be separately printed, mounted on boards, and hung up in schools. The suggestion was a good one, and I have even had thoughts of doing it myself. A publisher might take up that suggestion, and might issue those independently of me, and diminish what small sale I now have. Again, the work is very cumbrous and awkward; that can hardly be helped; but a publisher might see that the extracts arranged in ordinary volume form would be

valuable by themselves apart from the tables, and might get a good sale independently; and again my small returns would be cut into.

(*Sir H. Holland.*) That objection of yours would be partly met by the suggestion of Mr. Macfie, who brought this question of royalty before us, because his suggestion is, that no reprint is to differ from the original edition without the author's consent, either in the way of abbreviation, enlargement, or alteration of the text. Therefore, under that regulation, if that is carried out, a publisher could not print half of this book without your consent?—That would so far, if it can be practically worked out, meet my objection.

(*Mr. Trollope.*) But you have stated that you thought yourself of using this form of abridgment to which allusion is made?—I have.

And if this form of abridgment when made by you could be republished again by anybody else, then your profit would be interfered with?—No doubt of it.

(*Chairman.*) Supposing the suggested system of short copyright and royalty had been in force, would you have undertaken these works to which you have referred?—Certainly not. The enterprise was an unpromising one, pecuniarily considered, and it would have been almost an insane one, I think, had there not been the possibility of eventually getting back some returns from sales that were necessarily very slow. Moreover, the hopes under which the compilers have worked I could never have given to them.

Then are we to gather from your evidence that the system of short copyright and royalty would be injurious to the books of the graver class which do not appeal to the popular tastes?—I think so; it would be especially injurious to that particular class which of all others needs encouragement.

(*Sir H. Holland.*) As requiring most thought and

brain work on the part of the author?—Yes, and being least remunerative.

(*Chairman.*) I understand you to say that in all these cases you have not parted with the copyright yourself?—No, I have not.

Now assuming that the authors of these graver books sold their copyrights, do you think this royalty system would still act prejudicially upon them?—I think very decidedly. I have understood that it is contended that authors who sell their copyrights would not be affected by this arrangement. One of the answers I heard given here to-day sufficed to show that that is not true; inasmuch as a publisher who had to meet these risks would not give as much for copyright as he would otherwise give. His argument would be unanswerable. He would say, “Your book is a success, or not a success; if not a success, I lose what I give you for copyright; if a success, I shall have it reprinted upon me, and again I shall lose what I give you for copyright. I must, therefore, reduce the amount which I give for the copyright.” Moreover, I believe that the reduction in the value of copyright would be much greater than the facts justified. In the first place, the publisher himself would look to the possibility of reprinting with a fear beyond that which actual experience warranted. Frequently a suggested small danger acts upon the mind in a degree out of all proportion to its amount. Take such a case as the present small-pox epidemic, in which you find that one person in 30,000 dies in a week; in which, therefore, the risk of death is extremely small. Look at this actual risk of death and compare it with the alarms that you find prevailing amongst people. It is clear that the fear of an imagined consequence of that kind, is often much in excess of the actual danger. Similarly, I conceive that the publisher himself would unconsciously over-estimate the danger of reprints. But beyond that he would exag-

gerate his over-estimate as an excuse for beating down copyright. He would say to the author, "You see this danger; I cannot face so great a risk without guarding myself; and you must submit to a large reduction."

The evidence as continued on March 20 was as follows :

(*Chairman.*) I will ask you if you have any explanations you wish to offer on any point connected with the evidence which you gave on the last occasion?—Yes; I have to rectify some misapprehensions. From the re-statement made by Mr. Farrer, it would appear that in discussing the question of profits from re-publication of one of my works, I said I had "found that no other publisher would undertake the work without an additional profit of 10 per cent.," which implies that I had endeavoured to obtain another publisher. My meaning was that I ascertained that any other publisher who thought of issuing a rival edition, would expect to make a profit of 10 per cent. beyond the 10 per cent. commission for doing the business. Further, I have to remark that the case I took as illustrating the improbability that I should obtain any considerable compensation from increased sales under the royalty system, was the case of one of my works only, the "Principles of Psychology", and in respect of this, I may admit that there would be little danger of a rival edition. But it is not so with others of my works — with the work on "Education," now in its fourth thousand; with "First Principles," now in its fourth thousand, and especially with the just-issued first volume of the "Principles of Sociology." These are now sufficiently in demand, and, especially the last, sufficiently popular in manner and matter, to make rival editions quite probable.

Now, with respect to the stereotype plates, would they not enable you to exclude the rival edition of which you

speak? — I think not. In the first place, the assumption that other publishers would be deterred from issuing rival editions by my stereotype plates, implies that other publishers would know I had them. I do not see how other publishers are to know it, until after I had myself printed new editions — even English publishers, and it is out of the question that colonial publishers should know it. Hence, therefore, the fact of my having stereotype plates would not prevent such rival editions. Consequently these rival editions, making their appearance unawares, would compete with my existing stock, printed in a comparatively expensive style, and would oblige me either to sacrifice that stock, or to lower the price to one far less remunerative. Then, subsequently, there would not be the supposed ability to compete so advantageously with editions published by others. An edition to be sold at a cheap rate must not be in large type, well spaced, and with ample margins, but must be in small type, and much matter put into the page. Hence the existing stereotype plates, adapted for printing only books in a superior style, could not be used to print cheap books: the quantity of paper and the cost of printing would be much larger items than to one who arranged the matter fitly for a cheap edition.

Then are we to gather that you do not think that from any such cheap edition you would derive a profit from the royalty compensating you for your loss? — Nothing like compensating. Although the sales of these more readable books I have instanced might be considerably increased, the increase could not be anything like as great as would be required to produce the return I now have. Even supposing the price of the rival edition were the same, which of course it would not be, the 10 per cent. royalty would bring in the same amount, only supposing four times the number were sold that I sell now; and as, by the hypothesis, the price of the volume, to get any

such larger sale, must be much lower, the royalty would bring in so much the less. If, say, "First Principles" were issued at half the present price, 8,000 would have to be sold instead of 1,000, to bring in by royalty the present returns. Such an increase of the sale would be out of the question; even one half of it would be improbable so that certainly one half of my returns would be lost.

Have you any other personal experience that you wish to bring before the Commission to show that such a modification of the copyright law as you have been discussing would be disadvantageous to literature of the graver kind?—I think I have. "First Principles" was published in 1862, and in the course of some years the doctrine it contains underwent, in my mind, a considerable further development, and I found it needful to re-organise the book. I spent five months in doing this; cancelled a large number of stereotype plates; and was thus at considerable cost of time and money. As I have already pointed out, the work being now in its fourth thousand, has had a degree of success such that there might, under the proposed arrangement, very possibly have been a rival edition at the time I proposed to make these alterations. Had there been such a rival edition, this cost of re-organisation to me would have been more serious even than it was; since the difference between the original and the improved edition, adequately known only to those who bought the improved edition, would not have prevented the sale of the rival edition; and the sale of the improved edition would have greatly diminished. In any case the errors of the first edition would have been more widely spread; and in the absence of ability to bear considerable loss, it would have been needful to let them go and become permanent. A kindred tendency of the arrest of improvements would occur with all scientific books and all books of the higher kind, treating of subjects in a state of growth.

With the object of rendering useful books as accessible as possible to the public, do you think that those engaged in their production and distribution should be restrained from making what might be called undue profits?—In answer to the first part of the question I hope to say something presently, showing that the advantage of increased accessibility of books is by no means unqualified; since greater accessibility may be a mischief, if it tells in favour of worthless books instead of valuable books. But passing this for the present, I would comment on the proposition, which I perceive has been made before the Commission, that it is desirable to secure for books “the cheapest possible price consistent with a fair profit to those concerned.” I here venture to draw a parallel. What is now thought so desirable respecting books, was in old times thought desirable respecting food — “the cheapest possible price consistent with a fair profit to those concerned.” And to secure this all-essential advantage, more peremptory, indeed, than that now to be secured, there were regulations of various kinds extending through centuries, alike in England and on the Continent,—forbidding of exports, removing of middlemen, punishing of forestallers. But I need hardly recall the fact that all these attempts to interfere with the ordinary course of trade failed, and after doing much mischief were abolished. The attempt to secure cheap books by legislative arrangements, seems to me nothing less than a return to the long-abandoned system of trade regulations; and is allied to the fixing of rates of interest, of prices, of wages. In the past it was the greediness of money-lenders that had to be checked, or, as in France for many generations, the greediness of hotel-keepers; and now it appears to be the greediness of book-producers that needs checking. I do not see, however, any reason for believing that regulations made by law to secure cheap bread for the body having failed, there is likelihood of success for regula-

tions aiming to secure cheap bread for the mind.

Then do we understand you to mean that no analogy furnished by past experience in commercial affairs can be held to imply that the proposed royalty plan would succeed?—I think that all the facts are against it. I find it stated in the evidence lately given that there has not been raised “an insuperable objection in point of principle” to the plan of a royalty. If no such objection in point of principle has been raised, I think one may be raised; the objection, namely, that it is distinctly opposed to the principles of free trade. One of the aims of the plan, as expressed in the words of the same witness, is the “preservation of a fair profit to the author.” Now, on the face of it, it seems to me that any proposal to secure fair profits by legislation, is entirely at variance with free trade principles, which imply that profits are to be determined by the ordinary course of business. But further, I would point out that if it is competent for the legislature to say what is a “fair profit to the author,” I do not see why it is not competent for the legislature to say what is a fair profit to the publisher; indeed, I may say that it is not only as competent but much more competent. I take it to be impossible for the legislature to fix with anything like equity the profit of authors, if profit is to bear any relation to either skill or labour, as it should do; inasmuch as one author puts into a page of his book ten times as much skill as another, and, in other cases, ten times as much labour as another. Hence therefore, if they are to be paid at the same percentage on the price, there is no proportion in that case secured between the value of the labour and what they receive. Similarly, if we consider the numbers sold, the royalty which might afford ample return to an author who sold a popular book in large numbers would afford little return to an author who produced a grave book selling in small numbers. Obviously then it is extremely difficult,

and in fact impossible, for the legislature to fix an equitable royalty; but it is by no means so difficult for the legislature to fix an equitable rate of profit for the publisher. The function of the publisher is a comparatively mechanical and uniform function: the same practically for all books, the same for all publishers, and hence is a thing very much easier to estimate in respect of the proportion; and in fact we have the evidence that it can be fixed with something like fairness, inasmuch as publishers themselves voluntarily accept a 10 per cent. commission. Hence, I say, not only does the carrying out of the principle imply that if, in pursuit of alleged public advantage, the profit of the author should be fixed, then also should the profit of the publisher be fixed, but that it is much easier to do the last than to do the first. If so, then, it is competent for the legislature to go a step further. If there is to be a Government officer to issue royalty stamps, there may as well be a Government officer to whom a publisher shall take his printer's bills, and who adding to these the trade allowances, authors' 10 per cent. royalty, and publishers' 10 per cent. commission, shall tell him at what price he may advertise the book. This is the logical issue of the plan; and this is not free trade.

(*Sir H. Holland.*) You will hardly contend that the system of royalty is less in accord with free trade than the existing system of monopoly; you will not carry it so far as that, will you?—I do not admit the propriety of the word "monopoly."

Without using the word "monopoly," let me say, than the present system of copyright for a certain term of years?—I regard that as just as much coming within the limits of free trade as I hold the possession, or monopoly, of any other kind of property to be consistent with free trade. There are people who call the capitalist a monopolist: many working men do that. I do not think he is

rightly so called; and similarly if it is alleged that the author's claim to the product of his brain-work is a monopoly, I do not admit it to be a monopoly. I regard both the term "free trade" as applied to the unrestrained issue of rival editions, and the term "monopoly" as applied to the author's copyright, as question-begging terms.

Without saying what opinion I hold upon the point, and avoiding the use of the words "monopoly" and "free trade," I wish to know whether you think it most consistent with the doctrines of political economy, that every person should be able, upon payment, to publish a particular book, or that only one person should have it in his power to do so for a certain time?—Every person is allowed and perfectly free to publish a book on any subject. An author has no monopoly of a subject. An author writes a novel; another man may write a novel. An author writes a book on geology; another man may write a book on geology. He no more monopolises the subject than any trader who buys raw material and shapes it into an article of trade is a monopolist. There is more raw material which another man may buy. The only thing that the author claims is, that part of the value of the article which has been given to it by his shaping process; which is what any artizan does. The way in which this position of authors is spoken as "monopoly" reminds me of the doctrine of Proudhon—"Property is robbery." You may give a stigma to a thing by attaching to it a name not in the least appropriate.

(*Mr. Trollope.*) I understand your objection to a system of royalties to be this, that no possible *quota* that could be fixed would be a just payment for all works?—That is one objection. There is no possibility of fixing one that would apply to all works, inasmuch as the thing paid for is an extremely variable thing, more variable than in almost any other occupation.

I put that question to another witness before you, but.

I am afraid failed to make him understand me. I am therefore glad to have the answer from you in order that we may show (I think you will agree with me) that no special royalty specified by Act of Parliament could be just to poetry, and to the drama, and to fiction, and to science, and to history at the same time? Quite so. I think it is obvious, when it is put clearly, that it cannot be; and that is an all-essential objection.

(*Sir H. Holland.*) Nor would it in your opinion be desirable that the question of determining what amount of royalty is proper in each case should be vested in some registrar or some single person?—It would make the matter still worse. It would be bad to vest it anywhere, but especially bad to vest it in any single official.

(*Chairman.*) Are we to assume that you think the plan of a royalty to be at variance with the established principles of the science of political economy?—I think quite at variance with the principles of political economy. The proposal is to benefit the consumer of books by cheapening books. A measure effecting this will either change, or will not change, the returns of these engaged in producing books. That it will change them may be taken as certain: the chances are infinity to one against such a system leaving the returns as they are. What will the change be? Either to increase or decrease those returns. Is it said that by this regulation the returns to producers of books will be increased, and that they only require forcing to issue cheaper editions, to reap greater profit themselves, at the same time that they benefit the public? Then the proposition is that book-producers and distributors do not understand their business, but require to be instructed by the State how to carry it on more advantageously. Few will, I think, deliberately assert this. There is, then, the other alternative: the returns will be decreased. At whose expense decreased,—printers', authors', or publisher's? Not at the expense of the print-

ers: competition keeps down their profits at the normal level. Scarcely at the cost of the authors; for abundant evidence has shown that, on the average, authors' profits are extremely small. Were there no other motive for authorship than money-getting, there would be very few authors. Clearly, then, the reduction of returns is to be at the cost of the publisher. The assumption is that for some reason or other, the publishing business, unlike any other business, needs its returns regulating by law. Thinking, apparently, of prosperous publishers only, and forgetting that there are many who make but moderate incomes and very many who fail, and thinking only of books which sell largely, while forgetting that very many books bring no profits and still more entail loss, it is assumed that the publishing business, notwithstanding the competition among publishers, is abnormally profitable. This seems to me a remarkable assumption. Embarking in the business of publishing, like embarking in any other business, is determined partly by the relative attractiveness of the occupation and partly by the promised returns of capital. There is no reason to think that the occupation of publishing differs widely from other occupations in attractiveness; and hence we must say that, competing for recruits with many other businesses, it must on the average, offer a like return on capital. Were it found that the average return on capital in publishing was larger than in other businesses, there would immediately be more publishers; and competition would lower the returns. If, then, we must infer that, taking the returns of all publishers on the average of books, their profits are not higher than those of other businesses; what would be the effect of such a measure as that proposed, if, as anticipated, it lowered publishers' returns? Simply that it would drive away a certain amount of capital out of the publishing business into more remunerative businesses. Competition among publishers would

decrease; and as competition decreased, their profits would begin to rise again, until, by and bye, after a sufficient amount of perturbation and bankruptcy, there would be a return to the ordinary rates of profit on capital, and the proposed benefit to the public at the cost of publishers would disappear.

Then, with a view to the permanent cheapening of books, we may gather that your opinion is that it would not be effected in the way suggested?—I think not. The natural cheapening of books is beneficial; the artificial cheapening mischievous.

May I ask you to explain what you mean by contrasting the natural and the artificial cheapening of books?—By natural cheapening I mean that lowering of prices which follows increase of demand. I see no reason, *à priori*, for supposing that publishers differ from other traders in their readiness to cater for a larger public, if they see their way to making a profit by so doing; and, *à posteriori*, there is abundant proof that they do this. The various series of cheap books, bringing down even the whole of Shakespeare to a shilling, and all Byron to a shilling, and each of Scott's novels to sixpence, sufficiently prove that prices will be lowered in the publishing trade if the market is adequately extensive, just as in any other trade. If it be said that in this case authors have not to be paid, I would simply refer to such a series as that of Mr. Bohn, who, notwithstanding the payments to translators and others, published numerous valuable books at low rates. Moreover, we have conclusive evidence that with the works of still-living authors the same thing happens, when the market becomes sufficiently large to make a low price profitable. Witness not only the cheap editions of many modern novels, but the cheap editions even of Mr. Carlyle's works, and Mr. Mill's works. Deductively and inductively, then, we may say that there is a natural cheapening of books, going as far as trade

profits allow; as there is a natural cheapening of other things. Conversely, I mean by artificial cheapening, that kind which is anticipated from the measure proposed; for it is expected by means of this measure to make publishers issue books at lower rates than they otherwise do. And this is essentially a proposal to make them publish at a relative loss. If, as already argued, the average rates of publishers' profits are not above those of ordinary business-profits, these measures for lowering their prices, must either drive them out of the business or be inoperative. To put the point briefly—if there is an obvious profit to be obtained, publishers will lower their prices of their own accord; and the proposed competitive system will not make profits obvious where they were not so before.

But if there was free competition on the payment of the author's royalty, might it not be that another publisher would be led to issue a cheap edition when the original publisher would not?—I see, no reason to think this. The assumption appears to be that everybody but author and original publisher can see the advantage of a cheap edition, but that author and original publisher are blind. Contrariwise, it seems to me that the original producers of the book are those best enabled to say when a cheap edition will answer. The original producers of the book know all the data—number sold, cost, return, and so forth; and can judge of the probable demand. Another publisher is in the dark, and it does not seem a reasonable proposition that the publisher who is in the dark, can best estimate the remunerativeness of a cheap edition. If it is hoped that, being in the dark, he may rashly venture, and the public may so profit, then the hope is that he may be tempted into a losing business. But the public cannot profit in the long run by losing businesses.

(*Sir H. Holland.*) Take the "Life of Lord Macaulay"; you know that Tauchnitz has published a cheap edition

in four volumes,—a very neat edition, good paper and good print. Is it not possible that if this system of royalty is introduced, without considering whether the author would lose by it, a cheap edition like that would be put upon the market at once, and would pay the publisher?—It is possible that it would be done earlier than it is now done. I take it that the normal course of things is that, first of all, the dear edition should be published and have its sale, and supply its market, and that then, when that sale has flagged, there should come the aim to supply a wider market by publishing a cheap edition.

You are aware that one of the advantages which the advocates of this royalty system most strongly dwell upon, is that under the present system the great mass of the reading public are not able to purchase the books; those who have the advantage of circulating libraries can get them and read them, but poorer persons can neither purchase nor read them, whereas under the other system an edition like Tauchnitz would be at once put out, and it is contended that this, though it might be a loss to the author, would be a benefit to the public?—Then I take it that the proposal really amounts to this, that whereas, at present, the poorer class of readers are inconvenienced by having to wait for a cheap edition a certain number of years, they shall, by this arrangement, be advantaged by having a cheap edition forthwith; which is to say that people with smaller amounts of money shall have no disadvantages from their smaller amounts of money. It is communistic practically: it is simply equalising the advantages of wealth and poverty.

(*Chairman.*) Then we may assume that in your opinion the royalty system would not operate in cheapening books in the long run?—I think that in the first place, supposing it should act in the manner intended, by producing rival editions, it would act in cheapening just that class of books which it would be a mischief to cheapen. I have

already intimated in a previous reply, that the alleged advantage of cheapening books is to be taken with a qualification; inasmuch as there is a cheapening which is beneficial and a cheapening which is injurious. And I have got, I think, pretty clear evidence that the class of books cheapened would be a class which it is undesirable to cheapen. Being one of the committee of the London Library, I have some facilities for obtaining evidence with regard to the circulation of various classes of books; and I have got the librarian to draw me up what he entitles—"Recorded circulation of the following books during the three years following their introduction into the London Library." Here, in the first place, is a book of science—Lyll's "Principles of Geology"; that went out 28 times. Here, on the other hand, is a sensational book,—Dixon's "Spiritual Wives"; that went out 120 times. Here, again, is a highly instructive book,—Maine's "Ancient Law"; that went out 29 times. Here is a book of tittle-tattle about old times,—"Her Majesty's Tower"; that went out 127 times. Here, again, is another book of valuable inquiry,—Lecky's "European Morals"; that went out 23 times. Here is a book of gossip,—"Crabb Robinson's Diary"; that went out 154 times. Lecky's "History of Rationalism" went out 13 times; Greville's "Memoirs" went out 116 times. Herschel's "Astronomy" went out 25 times; Jesse's "George the Third" went out 67 times. I have added together these contrasted results, and the grave instructive books, taken altogether, number 118 issues, while the sensational and gossiping books number 584 issues; that is to say, more than five times the number of issues. Now, the London Library is, among circulating libraries at least, the one which is of all the highest in respect of the quality of its readers: it is the library of the *élite* of London. If, then, we see that there go out to these readers five times as many of these books which minister to the craving for excitement, and are

really dissipating books, as there go out the grave, serious instructive books, we may judge what will be the proportion of demand for such books in the public at large. Now let us ask what a publisher will do in face of these facts. He knows what these demands are; and he has to choose what books he will reprint. A publisher who has laid himself out for rival editions is comparatively unlikely to choose one of the really valuable books, which needs more circulating. I will not say he will never do it. He will do it sometimes; but he will be far more likely to choose one of these books appealing to a numerous public, and of which a cheap edition will sell largely. Hence, therefore, the obvious result will be to multiply these books of an inferior kind. Now already that class of books is detrimentally large: already books that are bad in art, bad in tone, bad in substance, come pouring out from the press in such torrents as to very much submerge the really instructive books; and this measure would have the effect of making that torrent still greater, and of still more submerging the really instructive books. Therefore, I hold that if the stimulus to rival editions acted as it is expected to act, the result would be to multiply the mischievous books.

(*Mr. Trollope.*) Do you not think that in making the parallel that you have there made you have failed to consider the mental capacities of readers?—I was about, in answering the next question, to deal indirectly with that; pointing out that while there is a certain determining of the quality of reading by the mental capacity, there is a certain range within which you may minister more or you may minister less. There are people who, if they are tempted, will spend all their time on light literature, and if they are less tempted will devote some of their time to grave literature. Already the graver books, the instructive books, those that really need circulating, are impeded very much by this enormous soli-

citation from the multitude of books of a gossipy, sensational kind. People have but a certain amount of time, and a certain amount of money, to spend upon books. Hence what is taken of time and money for instructive books is time and money taken away from the instructive; and I contend that if there were a diminution in the quantity of the books of this sensational kind published, there would be a larger reading of the really instructive books; and that, conversely, the multiplication of this class of lighter books would tend to diminish the reading of instructive books. I am now speaking, not, of course, of the higher amusing books, because there are many that are works of value, but of the lower novels, Miss Braddon's and others such.

Do you think that a man coming home, say, from his 8 or 10 hours labour in court day after day is in a condition to read Lyell's Geology as men read one of Miss Braddon's novels? We are speaking of some ordinary man.—No, not an ordinary man, certainly.

Have we not to deal with literature for ordinary men?—For both ordinary and extraordinary men; the whole public.

Are not the ordinary men very much the more numerous?—Certainly.

Is it not, therefore, necessary to provide some kind of literature, as good as you can, but such that the ordinary mind can receive and can turn into some profit, together with the normal work of life?—I am not calling into question in the least the desirableness of a large supply of literature of an enlivening and amusing and pleasant kind, as well as a large supply of graver literature. My remarks point to the literature that is neither instructive nor æsthetic in the higher sense, but which is bad in art, bad in tone, worthless in matter. There is a large quantity of that literature, and that literature I take to be the one which will be the most fostered by the proposed

measures. I do not in the least reprobate the reading of lighter works if they are good in quality. I refer to the class of works which I regard as not good in quality.

But do not you think you must leave that to settle itself on those principles of free trade which you have just enunciated so clearly?—Certainly; I am objecting to a policy which would tend to encourage the one and not encourage the other.

(*Sir H. Holland.*) The subscribers to the London Library are, as you say, the *élite* of readers?—Yes.

And is not that the reason why there is this difference as to the reading of good and bad books taken out from that library; is it not attributable to the fact that these people have probably bought and have in their own houses the good books, but that they want to look through these other books, and therefore get them from the library?—There may be a qualification of that kind; but inasmuch as a very large proportion of the readers of the London Library are ladies, and those who come for lighter literature, I do not think it at all probable that they would have bought Lecky or Maine, or any books of that kind.

I ask the question because I rather think that you will find a very curious difference from that which you have been stating if you go to the Manchester and Liverpool free libraries. You will find there that the working men take out largely Macaulay's "History of England" and that class of book?—Well, whatever qualifications may be made in this estimate, or the inferences from this estimate, I do not think they can touch the general proposition that books of this kind which in the London Library circulate most largely, are books of the kind which circulate most largely among the general public, and books of the kind which a publisher of rival editions would choose. That is my point.

But might not that very evil to which you refer be met by improving the taste of the majority of the poorer

readers, by enabling them to get at once cheap editions of good books?—The question is, which are the cheap editions that will be issued. I contend that they are the cheap editions of these books of a dissipating kind; and that the main effect will be to increase the dissipation.

You do not think that the earlier publication of a cheap edition would raise the tone of readers?—I do not see that it would do so, unless it could be shown that that would tell upon the graver and more instructive books. My next answer, I think, will be an answer to that.

If you improve the tone of the readers, of course it does tell upon the graver books for those who have time to read the graver books; but there is a large class of readers who have not that time?—Yes.

(*Chairman.*) Referring to the illustrations which you have just given of works which you would denominate as worthless, or comparatively valueless, did I hear among them historical memoirs and journals?—"Crabbe Robinson's Diary," for instance; I call that a book of gossip which anybody may read and be none the better for it.

The question I should like to ask is, are you not of opinion that books of that sort are extremely valuable to the intending historian of the epoch to which they refer?—It may be that there are in them materials for him. I have not read the "Greville Memoirs" myself, and I have no intention of reading it; but my impression is that the great mass of it is an appeal to the love of gossip and scandal, and that it is a book which, if not read at all, would leave persons just as well off or better.

Take "Lord Hervey's Memoirs," in the reign of George the Second; if you had the privilege of reading that book you would probably say it was an extremely sensational book, but knowing the position which Lord Hervey occupied in the Court and family of George the Second, I presume we may take for granted that the extraordinary facts which he relates are facts; and if so they

would form the basis of a great deal of truthful history, which would be written of that reign; would not that be so?—It might be so, no doubt.

Then we understand you to mean that in your opinion the royalty system would not cheapen works that you would describe as valuable?—I think, on the average of cases, quite the contrary. I believe the system would raise the prices of the graver books. Ask what a publisher will say to himself when about to publish a book of that kind, of which he forms a good opinion. “I have had a high estimate given of this book. The man is a man to be trusted; the book possibly will be a success. Still my experiences of grave books generally, are such that I know the chances are rather against its succeeding. If it should be a success, and if I had ten years now to sell the edition, I might print 1,000; but, under this arrangement, a grave book not selling 1,000 in three years, or anything like it, it will never do for me to print 1,000. Should it be much talked about by the end of the three years, there might be a rival edition, and my stock would be left on my hands. Hence, now that there is this very short time in which I can sell the book, I must print a smaller number—say 500. But if I print 500 and expect to get back outlay and a profit on that small number, I must charge more than I should do if I printed 1,000 and had time to sell them. Therefore the price must be raised.” In the case of a book which did turn out a success, it might eventually happen that there would be a cheap edition issued, and that that raised price would not be permanent; but this argument of the publisher with himself, would lead him to raise the price, not only of that book, but of the other grave books which he published, all of which would stand in the same position of possibly being successes, but not probably; and of these, the great mass, the nine out of ten that did not succeed. the price would remain higher,—would never be lowered.

There would not only be that reason for raising the price : there would be a further one. If a man in the wholesale book-trade, who puts down his name for a certain number of copies, knows that a cheaper edition will possibly come out by-and-bye, the result will be that he will take a smaller number of copies than he would otherwise do. At the beginning he may take his 25 or 13, as the case may be ; but as the end of the three years is approaching he will say, "No, I will not take a large number ; I must take two or three." Then, still further, the reader himself will be under the same bias. He will say—"Well this book is one I ought to have : I hear it highly spoken of, but it is probable that there will be by-and-bye a cheap edition ; I will wait till the end of the three years." That is to say, both wholesale dealers and readers would earlier stop their purchases, thinking there might be cheap edition ; and that would further tend to diminish the number printed and to raise the price.

(*Sir H. Holland.*) Might it not be that the publisher, instead of entering into those calculations that you have pointed out, would consider, knowing that other editions may appear, "What is the cheapest form in which I can print this book? What can I afford to give the author consistently with bringing out the cheapest possible book, so that I may be secure against any other publisher bringing out a cheaper edition"?—It would be a very reasonable argument, if he knew which, out of these various books of the graver kind, was going to succeed ; but since nine out of ten do not succeed—do not succeed, at least, to the extent of getting to a second edition—do not succeed, therefore, so far as to make it at all likely that there would be a rival edition, and that a cheap edition would pay, he will never argue so ; inasmuch as he would in that case be printing, of the nine books that would not succeed sufficiently, a larger edition than he would ever sell. He must begin in all these cases of doubtful grave

books by printing small editions.

Where an author brings a book to a publisher, the first question the publisher asks himself is, of course, this, "Is this book likely to take?" and then if he thinks it will take, he has to consider further, in what degree will it take? Will it have a large sale or limited sale? Because, in each case the book may be a success, though in a different degree. Then, if it is competent for any other publisher to publish an edition, it may be assumed that such edition would be a cheap one; and, therefore, has not the original publisher this further question to put to himself: "The book, I think, will take, but looking to the chances of a cheaper edition, I must see what compensation I can give to the author, publishing this book as cheap as possible, so that I may not be underbid hereafter"?—But, I think, that the experiences of publishers show that it does not answer their purpose to run the risk of cheap editions with the great mass of graver books; inasmuch as nine out of ten of them do not pay their expenses—and do not pay their expenses, not because of the high price, but because they do not get into vogue at all. The publisher would argue—"It will never do to print cheap editions of all these ten because one out of the number will succeed."

Of course he does not do so now, because there is not any possibility of another publisher underbidding him by a cheap edition; but I am assuming a case where any publisher, on payment of a royalty, can publish a cheap edition: then the original publisher would have to consider, "How cheaply can I publish this edition so that I may not be underbid by another publisher"?—That, I say, would altogether depend upon the experience of the publishers as to what was, in the average of cases, the sale of a new book. In most instances the sale of a new grave book is very small — not sufficient to pay the expenses; and I think the publisher would make

a great mistake if, in the case of such a book, he counted upon getting a large sale at once by a low price. The other argument would, it seems to me, be the one he would use. In fact, I not only think so, but I find my publishers think so.

(*Chairman.*) Do you wish to instance any particular case in which you believe that a fixed royalty, such as we heard about, would have hindered the diffusion of a book of permanent value?—Yes; I have an extremely striking and, I think, wholly conclusive, instance of the fatal effects,—the extensive fatal effects,—that would have resulted had there been any such system existing as that proposed. I refer to the “International Scientific Series.” I happen to know all about the initiation of that. It was set on foot by an American friend of mine, Professor Youmans, who came over here for the purpose. I aided him, and know the difficulties that were to be contended with, and a good deal concerning the negotiations. The purpose was to have a series of books written by the best men of the time, in all the various sciences, which should treat of certain small divisions of the sciences that are in states of rapid growth—giving to the public, in popular form, the highest and latest results; and it was proposed, as a means of achieving this end, that there should be an international arrangement, which should secure to authors certain portions of profits coming from translations, as well as profits from originals at home, and the hope was that some publisher might be obtained who would remunerate these authors of the highest type at good rates, so as to induce them to contribute volumes to the series. Well, this attempt, after much trouble, succeeded. A number of the leading scientific men of England, France, and Germany were induced to co-operate. A publisher was found, or rather publishers here and elsewhere, to enter into the desired arrangements; and an English publisher was found who offered such terms

to authors in England as led men in the first rank (and I may mention Professor Huxley, and Professor Tyndall, and Professor Bain, and Professor Balfour Stewart, and a great number of others) to promise to write volumes. These men, I know, were reluctant, as busy men, with their many avocations, and their incomes to get for their families, would naturally be, and were induced to enter into the scheme only on its being made manifest to them that they would reap good profits. The English publisher offered a 20 per cent. commission on the retail price, paid down on first publication, and for every subsequent edition paid six months after date; and there were certain smaller percentages to come from abroad. Now, the English publisher proposed to give those terms, knowing that it would be impossible for him to get back his outlay unless he had a number of years in which to do it. He had to stereotype, he had to pay at once these sums to authors, and he had to publish the books at a cheap rate; for, by the way, I ought to have said that part of the plan was that these books should be sold at low prices: I may instance a volume of 420 pages for 5*s*. These terms would, I take it, have been absolutely out of the question had there been such an arrangement as that under which the publisher, instead of having many years to recoup himself, would have had rival editions to compete with in the space of three years. I do not, however, put that as an opinion. I have taken the precaution to obtain from Mr. King, the publisher, a definite answer on the point. This is the paragraph of his letter which is specially relevant:—"Authors can have no difficulty in proving that this" (meaning the system which I told him was proposed) "would be most unjust to them, a confiscation, in fact, of their property; but I, from a publisher's point of view, should like to declare that the terms on which my firm have undertaken the 'International Scientific Series' would be impossible on such a limitation." Now here, then, we

have a series of highly valuable books, I think of the kind specially to be encouraged, amounting to between 20 and 30 already published, and potentially to a much larger number, which would not have existed at all had there been in force the arrangement proposed; inasmuch as the publisher affirms that he would not have offered such terms, and I can testify that in the absence of terms as tempting as those, authors would not have agreed to co-operate.

(*Sir H. Holland.*) Was Mr. King made aware that there would be a limited time within which each volume would be protected?—Yes, three years. He did not count upon anything like adequate return in that time. He says—“We are a long way off profit as yet on the series” (I think it is nearly five years since it commenced), “although I am convinced that ultimately we and the authors, too, will be well satisfied.”

That would raise the question which I wanted to put, whether in a case like that it would have been possible to have published a cheaper edition than the one now published?—Yes, in the absence of the author’s 20 per cent.

In the case which you have brought to our notice may we assume that the cheapest form of edition was published consistently with fair profit to the author and publisher?—I think, certainly, with anything like a tolerable mode of getting up. Of course you may bring down a thing to rubbishing type and straw paper; but I was speaking of a presentable book. They are very cheap for presentable books.

That, perhaps, would be one of the evils arising from a system of royalty, that you would get extremely bad and incorrect editions published of a book, even in the first instance?—Very likely.

Because it would be the publisher’s object, if that system were thoroughly established, to publish such an

edition that another publisher could not underbid him at the end of the three years; that would be, would it not, the general object of the publisher?—Yes.

In this case I understand you to say that he could not, consistently with fair profits to the author and publisher, and consistently with its being a properly printed work, without which a work of that kind would be of very little value, have published a cheaper edition?—He could not.

And yet he would not have been able to publish such an edition if he had run the risk of being underbid?—Certainly not. He says—“I confess my idea in proposing such terms as those of the ‘International Scientific Series,’ looked forward to a yearly increasing interest in scientific literature, and an ever enlarging circle of readers able to appreciate books of a high class.” So he was looking for a distant effect.

I am anxious, as Mr. King is not here, to get your own opinion upon that point; do you concur in his views?—Yes, certainly.

(*Chairman.*) Have you any further reasons for thinking that measures of the kind which we have been discussing, taken in the interest of cheapening books, might end in doing the reverse?—I think there is another way in which there would be a general operation of this system of rival editions, which would have, indirectly, the effect of raising the prices all round; namely, the waste of stock. It would inevitably happen that every publisher of an original edition would, from time to time, have a rival edition make its appearance before his edition was sold. In that case his remnant of an edition got up in a relatively expensive style, would either have to be not sold at all or sold at a sacrifice. Further, it would happen from time to time that two publishers, unknown to one another, would issue rival editions, both of which would not be demanded; there would therefore be a waste

of stock. Evidently the system of competing with one another in the dark, would continually lead to production in excess of demand. What would be the result? If there is an increased per-centage of waste stock, that has somehow to be paid for, if business is to be carried on at all. And as we know that tradesmen have to raise their average prices to cover their bad debts; so, if publishers find an increase of bad stock they must raise their prices to cover the loss on bad stock.

(*Mr. Trollope.*) Would not the ordinary laws of trade correct such an evil?—This interference with the laws of trade would entail an abnormal production of waste stock. Under the present system a publisher does not publish a cheap edition till the other is gone; but under the proposed system, with cheap copies perhaps sent from the colonies, there must be waste stock.

When the system had been in operation for a time do you not consider that that evil would correct itself by the ordinary laws of trade. We are aware that at first the disruption of an existing state of things will create much confusion, and such evil as you have described; but are you not of opinion that this would rectify itself after a time?—I do not see how it could rectify itself, if the system of rival editions continued, and operated in the way that it is expected to do. But as I have already indicated by certain hypothetical remarks, I do not think it would continue and operate in that way. I say, however, that if rival editions were issued by men not knowing each others doings, there must from time to time occur in the business of each publisher loss of stock.

(*Chairman.*) From the answer to the last question that has been put by Mr. Trollope I gather it to be your opinion that the arrangement would be practically inoperative so far as the anticipated competition was concerned?—I think that after a period of perturbation, a period of fighting and general disaster in the publishing busi-

ness, there would arise a tacit understanding among publishing houses, which would, in a large degree, defeat the purpose of the measure; and I say this on the strength of definite facts furnished by trade-practices in America. These facts I have from the before-named American friend, Professor Youmans, with whom from time to time, when over here, I have had to discuss the probability of pirated editions of my own books in America. My books in America are published by a large house there, the Appletons; and they deal with me very fairly—pay me as well as American authors are paid. I have gathered from Professor Youmans that the danger of the issue of rival editions of my books in America is very small; because there exists among the American publishing houses, the understanding that when one house brings out an English book, other houses will not interfere: the mere circumstance of having been the first to seize upon a book, is held to give a priority, such as is tacitly regarded as a monopoly. That condition of things has been established through a process of fighting; for when it did at first happen that American houses brought out rival editions of the same English book, or one edition, rather, after another, that, of course, was a declaration of war between the two houses, and immediately there was retaliation, and it ended in a fight. The house attacked revenged itself by issuing, perhaps, a still cheaper edition, or by doing the like thing with some work subsequently published by the aggressing house; and after bleeding one another in this way for a length of time there resulted a treaty of peace, and a gradual establishment of this understanding, that they would respect each other's priorities. If that is what happened in America, when the only claim that a publisher had to the exclusive publication of a book was the claim established by prior seizing of it, of prior printing, much more will it happen here in England, among publishers who have paid for their books,

or who have entered into arrangements with authors for half profits, or what not. Having established certain equitable claims to these books they will very much more decidedly fight any houses that interfere with them, by issuing rival editions. If the men who have ill-founded claims fight, still more will the men who have well-founded claims fight. Hence, there would occur among the English publishers, when this system came into operation, a period of warfare lasting, probably, for some years, and ending in a peace based on the understanding that any publisher who had brought out a book would be regarded as having an exclusive claim to it, and would not be interfered with. The fear of retaliation would prevent the issue of the rival editions.

(*Sir Henry Holland.*) And therefore would prevent the publication by a rival publisher of a cheaper edition? —Yes.

(*Chairman.*) Then on the grounds that you have explained, you think the system would become before long wholly inoperative? Not wholly inoperative, I think: inoperative for good, not inoperative for evil. In the course of this early phase to be passed through, in which houses issued rival editions against each other and got into this state of warfare, it would happen that the weaker would go to the wall: the smaller publishers would not be able to stand in the fight with the larger publishers, and they would tend to fail. And further, although treaties of peace would be eventually reached between the more powerful publishers, who would be afraid of each other, and dare not issue rival editions of each others books, there would be no such feeling on the part of large publishers towards small publishers. If a small publisher happened to issue a successful book, a larger publisher would have no fear in issuing a rival edition of that. Hence, therefore, the tendency would be for the small publishers to be ruined from having their successful

books taken away from them. But that would not be the only tendency: there would be a secondary tendency working the same way. For after this fighting had gone on a year or two, it would become notorious among authors that if they published their books with small publishers they would be in danger of rival editions, in case of success, being issued by large publishers; but that, contrariwise, if they published with large publishers they would be in no danger of rival editions. Hence they would desert the small publishers; and in a double way the small publishers would lose their business. We should progress towards a monopoly of a few large houses; and the power which such have already of dictating terms to authors, would become still greater.

And if I understand you rightly, the power would be not only to dictate terms to authors but of price to the public?—Yes, they would be able to combine. When you got a small number of publishers, and they could agree to a system of terms: the public would be powerless against them, and authors would be powerless against them.

Then, in your opinion, is there any way by which works could be cheapened by legislative enactment?—There is one way, and that a way in principle exactly the reverse of that which is contended for in this measure; namely, the extension of copyright. I do not mean the extension in time; I mean the extension in area. On this point I am happy to say there appears to be agreement between the two sides. From the evidence which I have read I gather that it is proposed along with this limitation of copyright in time to extend copyright in area. I do not altogether understand the theory which, while it ignores an author's equitable claim to the product of his brain-work in respect of duration, insists upon the equity of his claim to that product of his brain-work, as extending not only to his own nation but to other nations. However, I

am glad to have agreement so far; and I hold, along with those who support the proposed measure, that the enlargement of the markets by means of international copyright would be a very effectual means of cheapening books. It would be a more effectual means of cheapening the best books. I may refer again to this International Scientific Series. One of the means by which that series has been made cheap, was, that the American publisher and the English publisher, agreed to share between them the cost of production, in so far as that the American publisher had duplicate stereotype plates and paid half the cost of setting up the type. Now it is clear that if the outlay is diminished by having one cost of composition for two countries instead of a cost for each, the book can be issued at a lower rate in both countries than it could otherwise be. And that arrangement which was voluntarily made, under a kind of spontaneous copyright, in the case of the International Series, would be forced, as it were, upon publishers in the case of an established copyright. Consequently there would be habitually an economization of the cost of production, by dividing it between the two countries; and hence there would be a lowering of the price. And then there is the further fact that this would tell especially upon the more serious books. On books of a popular kind the chief cost is for paper and print: large editions being printed. Therefore it does not so much matter in America having to set up the type afresh. But in the case of a grave book of which the circulation is small, the cost of composition is the main element in the cost; and the economization of that cost, by dividing it between England and America, would serve very considerably to lower the price.

(*Dr. Smith.*) Then, if I understand you aright, you do not approve of the principle adopted in the Canada Act, in the Act passed by the Canadian Legislature of 1875, confirmed by the Imperial Act, by which it is ne-

cessary in order to obtain copyright in Canada that the works should be set up afresh?—I think that it is obviously nothing else than a means of staving off the opposition of printers, and a very mischievous arrangement.

Would it not be the fact that if a work could be set up once for all in the country, and circulate in the two countries, the price of the book would be diminished?—Unquestionably.

(*Sir H. Holland.*) You are aware of the difficulties that have been raised by the United States publishers; that constant attempts have been made ever since 1854 and before to make a copyright convention, and that there is no very great probability of these attempts proving successful. Have you any particular suggestion to bring before the Commissioners which would in your opinion tend towards making the Americans favourable to a convention?—I am sorry to say I do not see my way towards any such suggestion. I am merely replying to the general question whether legislation could do anything to cheapen books, and saying that the only thing I thought it could do would be to get, in some way, an extension of area for copyright.

The following is the speech referred to above as having been made at a meeting of the National Association for the Promotion of Social Science.

With respect to the duration of copyright, I would remark first, that if any *reason* is to be given for fixing a term of years, a good one may be given for the Commissioners proposal; whereas, for the term proposed in this bill I see no reason: why fifty rather than sixty or forty should be fixed cannot be shown; but it may be shown why copyright for life and thirty years after death is reasonable. The author is a man carrying on a money-getting occupation, and should, if possible, be put in the position of feeling that he is doing as well as may be for

his family, and that he is not by following that occupation in place of another sacrificing them. If he is conscious that by pursuing authorship he runs the risk of leaving his children without any provision after his death, he may be led to think that duty to them should make him choose another occupation. But by making the duration of copyright for his life and thirty years afterwards, he is encouraged by a reasonable belief that he will leave means for supporting his family for a term sufficient to allow his children to be brought to a self-supporting maturity. Let me next refer to another reason given very clearly by Mr. Westlake for preferring a fixed termination of copyright after death, rather than a series of terminations of different dates, for the works published at different dates. It happens that I can give personal illustrations of the great inconveniences, and I may say mischiefs, which would arise from the termination of an author's copyrights at different dates. I was not aware until two days ago, when talking to Dr. Smith on this question, that the existing cheap edition of Hallam's *Middle Ages* is an imperfect work. I have been making quotations from that work. I shall now have to go back on my quotations and see if I have been betrayed into errors; and observe, further, that but for mere accident I should have been in the predicament of, perhaps, having quoted obsolete passages. I will give a second illustration, also personal, but in another way. In 1862 I published a work entitled *First Principles*. Although the ideas contained in it were true as far as they went, they were imperfectly developed and were imperfectly organised. That which was primary was put as secondary and *vice versa*. At the end of five years I published a second or a re-organised edition presenting the doctrine under quite a different aspect. Now what would happen in this case supposing copyrights terminated at the end

of 50 years? My edition of 1862 would be republished in a cheap form, while the re-organized edition was still copyright; and for the succeeding 5 years there would be a propagation of my erroneous views; and the imperfect edition, filling the market, would hinder the spread of the perfect edition when subsequently published. In brief I may say that this proposal is one which, if carried out, would establish a premium on the propagation of error. I pass now to the question of colonial reprints. If the clause which gives reprinting powers, under certain conditions, had been a clause in a bill proposed centuries ago, I should not have been surprised; but that such a clause should appear in a bill at the present time after the free trade doctrines have been established, is to me astounding. I read in this clause (the 72nd)—‘Whereas it is desirable to provide means whereby the inhabitants of all British possessions may obtain, at a moderate price, a sufficient supply of books.....’ Thus we have actually come back to the notion that it is the duty of the state to provide the colonies with a supply of a commodity at a moderate price. It is really a reversion to the form of legislation which in old times dictated the rates of wages, provided for the qualities and quantities of goods, entered into factories to inspect processes of production, established bounties and restrictions, and so forth. For all these things were done with the view of obtaining good supplies of commodities at reasonable rates. What can possibly be the defence for this revival of antique legislation? It is that though the state has proved a bad judge in respect of food and clothing, and things of daily use, it is likely to be a good judge in respect of literature? Is it that having failed in the relatively easy thing, it will succeed in the relatively difficult thing? Then, further, what is the particular authority which it is proposed to constitute the judge? The Governor of the colony. Who is he? Usually he is a

general. Is he a fit man to judge whether a certain book is adequately supplied in the colony, and whether such and such a price is a reasonable one for it? Then what is proposed by way of defence for the author? The author is to have due notice of the proposed reprint. He may, it seems, go personally and make objections; but what author will ever go to the colony to oppose? He may do it by his agent; but what agent has the author in a colony? He probably knows nobody there. Supposing he could find a fit agent, what likelihood is there of the cost of such a transaction ever being repaid, even supposing he succeeds in his opposition? The cost of the transaction would, probably, be more than the author would get for an edition of his work. Practically, therefore, the clause involves abolition of copyright in the colony; and we have good reasons for suspecting that the proposed royalty would bring next to nothing. Even supposing it should turn out that the arrangement worked as intended, I should still demur to the assumption that the colony would benefit. It continually happens in every kind of legislation that the unanticipated results immensely exceed in importance the anticipated results. We may suspect it would be so here. For what would be the books which a colonial publisher would be likely to reprint? Clearly the books which would repay him. What would they be? Why the novels of the day, the gossiping biographies, the books which feed the voracious appetite for personalities: those would be the books they would seize upon for the purpose of reprinting. But the books of an instructive kind, the books of small circulation, would not, in most cases, pay the expenses of their republication. But if, while you do not cheapen the instructive books, you do cheapen the amusing books, you make it easier for people to satiate their appetites upon these and diminish their appetites for the others. If those who have daily but a short time for reading can get easy

access to the one kind of literature, while the access to the other kind is difficult, they will be led to read more of the first than they would otherwise do, at the expense of the last. The consequence will be not an educating influence but an uneducating influence. Before closing, let me say a few words on the general question of copyright. There is a current belief, which was expressed before the commission, that copyright is an artificial arrangement—that it is some privilege granted by the state to secure the author a monopoly. I hold this be an erroneous view. If the state will in this matter do what the state has to do in all other matters of commerce, namely, enforce contracts, copyright comes into existence as a matter of course. In all other trading transactions, the law recognises contracts, both overt and tacit. It not only recognises those in which there has been an agreement by signature; it recognises those in which not even an oral agreement has been made. If a man goes into a shop and asks for a pound of tea or any other commodity, and it is handed over to him, it is not supposed to be requisite that he should specify beforehand that he will give so much money for it. That is to say, the state in these cases recognises the tacit contract to pay a price, though this has not been mentioned. What is the tacit contract with regard to a book? When the buyer of a book goes into a shop and buys from the author's agent, what is the contract entered into in the purchase of that book? The tacit understanding is that it is sold for the purpose of reading, either by the individual or other individuals, and for no other purpose. Ask what would happen if the purchaser announced that he was about to use that book for reprinting. Clearly it would either not be sold to him at all, or it would be sold at a relatively immense price—a price such as would cover the profit on the edition. If, therefore, the law is to enforce tacit contracts, it is to enforce this tacit contract, that the book

bought shall be for reading purposes and not for reprinting purposes; and if it enforces this tacit contract, copyright results as a consequence. I contend, then, that the state has nothing more to do in the matter than to make provisions for carrying out this tacit contract in all its details. A word as to the cheapening of literature. The true way to get cheap books is rigorously to abide by copyright as thus resulting, and to extend its area. Such extension of copyright as would bring under it a larger population, and therefore a larger number of purchasers, would make it possible to lower the prices of books; but if you narrow the area of the copyright and so diminish the number of purchasers of the book, you necessarily raise its price. This proposed arrangement, by which colonists are to have a cheaper book, will, by cutting off the colonial sale of the English edition, raise the price in England. Conversely, if copyright could be extended by including the United States, the prices of all books might be lowered. Where there is an agreement, as there frequently is already, between British and American publishers to share the cost of composition and stereotyping, the prices charged for books are reduced both here and in America. Under an international copyright this exceptional result would become a general result.

In reply to objections there were the following supplementary remarks.

It is rather odd that we should get to the abstract question at the end of our discussion, and not at the beginning. We should have settled the basis at first. I now find myself in the position of having to prove that copyright is not, as it has been called, a monopoly; and to prove that the maintenance of copyright is really free trade. I can hardly go into the matter adequately now; but I would point out that a monopoly, properly so-called, and free trade, properly so-called, have these charac-

ters. The monopolist is a man who stands in the way of some one who, in the natural order of things, would be able to carry on some business in his absence just as well as in his presence. The free-trader is one who needs no aid from the monopolist, but simply wishes to do that which he could do did the monopolist not exist. But one who, wishing to reprint an author's book, calls the author a monopolist for preventing him, stands in a widely different position. He proposes not simply to use his powers with the aid of such natural resources as are open to all. He proposes to use that which would not exist but for the author. It is, therefore, an utter misuse of the word to call the author's claim a monopoly. Moreover, those who so call it show the fallacy of their characterization by not daring to act upon it. Free trade makes no compromise with monopoly, rightly so-called. The free trader is ready to abolish monopoly at once, and makes no terms with it—sees no need for fostering it. Whereas those who take the position that copyright is a monopoly are obliged to admit that you must allow this so-called monopoly for a time. They dare not propose that the moment an author's book is published, any one should be allowed to reprint it; and they thus prove that they have not the courage of their opinions—do not really believe that which they profess to believe. On the other hand, I contend, as before, that freedom of trade is essentially freedom of contract, and that if authors, through their agents, are allowed to make what contracts they please with book-buyers, while the state stands by and enforces the contracts made, copyright necessarily comes into existence.

A REJOINDER TO MR. McLENNAN.

In Part III of the *Principles of Sociology*, dealing with "Domestic Institutions," I had occasion to criticize cer-

tain of the views set forth by Mr. McLennan in his *Primitive Marriage*. Sometime after, in two articles in *The Fortnightly Review*, the last of which appeared in June, 1877, he replied to my criticisms. To prevent prolongation of the controversy, the Editor of *The Fortnightly Review* sent me a proof of this last article; with the result that what I had to say in answer was appended. As Mr. McLennan's essays above named have, along with others, been put into a permanent form, it seems fit that permanence should be given to my response. The few pages occupied run as follows:—

Forms of family produced by descent in the male line, are habitually characterized by a law of succession which gives the sons of the eldest precedence over his brothers. Contrariwise, forms of family in which descent in the female line persists, wholly or partially, because paternity is unsettled or but partially settled, are characterized by a law of succession under which brothers take precedence of sons. Hence an institution which requires a younger brother to beget an heir for an elder brother who dies without one, and which thus carries to an extreme the claims of sons *versus* the claims of brothers, seems like a result of a family system characterized by established descent in the male line. Mr. McLennan, however, considers this peculiar institution to be derived from a form of family in which, from indefiniteness of paternity, male kinship in the descending line is imperfectly established. As he interprets the matter, cause and consequence stand thus:—"On every view, then," he says, "the succession of brothers in preference to sons must be accepted as a remainder of polyandry" (p. 705). Nevertheless he represents, as a remainder of polyandry, this

Levirate system, which gives such preference to sons that even the nominal son of the eldest brother excludes a younger brother.

Though Mr. McLennan thinks "it is impossible not to believe" that this is the origin of the Levirate (*Studies in Ancient History*, p. 162), I have ventured to suggest another possible interpretation. I have shown that where women are bought and sold as property, they are also inherited as property. I have given six cases where widows are inherited by brothers who claim them as well as other belongings of the deceased; and have pointed out that in two of these instances, the nearest relation "had a right" to the widow, in the absence of a brother. As further showing how transfers of widows are originally transfers of property, I have given six cases in which sons inherit their father's wives (save their own mothers).¹ Here let me add other instances having like implications. Speaking of the Kakhyens, Anderson, in his *Mandalay to Momien* (pp. 139—142), says, "the curious custom obtains that a widow becomes the wife of the senior brother-in-law, even though he be already married." And Wood tells us of the Kirghiz, that on a husband's death the wife goes to his brother, and on his decease becomes the property of the next of kin. We have, then, multitudinous proofs that the taking to wife deceased brothers' widows (not in these cases associated with polyandry, but with polygyny), is part of the succession to property in general; and this was originally the case among the Hebrews. The inference which Mr. McLennan draws from the ancient tradition concerning Tamar, does not correspond with the view which the Rabbins held respecting the original form of the Levir marriage. As shown by a passage in Lewis (*Origines Hebrææ*, ii. 498), the Rabbins saw in Levir marriage, essentially a right of the brother, not of the widow. At first sight it

¹) *Principles of Sociology*, i. 680.

is not manifest how what was originally a right of the brother, became transformed into a duty; but I have given some facts which throw light upon the transformation. Even among a people so little advanced as the Chippewas, the claim of a dead brother's wife as property, had so far changed that the assigned reason for marrying her was the obligation to take care of the brother's children; and I have cited the case of an Egyptian who said he married his brother's widow because "he considered it his duty to provide for her and her children." Following the clue given by these cases, I have suggested (*op. cit.* p. 692) that the duty of raising up seed to a dead brother was originally the duty of raising the seed the dead brother had left, that is, his children; and that this eventually passed by misinterpretation into the duty of preserving his line, not by rearing existing children, but by begetting a son in his name when he had none—a misinterpretation prompted by that intense craving to survive in name through future times, described in Psalm xlix. 11:—"Their inward thought is that their houses shall continue for ever..... They call their lands after their own names." When we remember that even now, estates are sometimes bequeathed on condition of adopting the name of the testator, and so nominally maintaining the line, we shall understand the motive which exaggerated the duty of raising a brother's heir until it became the duty of raising an heir to him. Should Mr. McLennan contend that this transformation of what was once a beneficial right into an injurious obligation is improbable, then I make two replies. The first is, that among many remarkable social transformations, there may be named one immediately relating to marriage-customs, which presents us with a no less complete inversion. Change from wife-purchase to the reception of a dowry with a wife, does not seem a change likely to result by gradual transition; yet it did so result. The property given for the bride,

originally appropriated entirely by the father, ceased in course of time to be wholly retained by him, and he gave part to his daughter for her special use after her marriage. What he gave to her grew, and what was paid for her dwindled, until eventually the husband's payment became a symbol, while the father's gift developed into a substantial dower. The second reply is that this transformation is less difficult to understand than the one alleged by Mr. McLennan. For according to him, the arrangement by which, in the polyandric family, an elder brother's death profits the next brother by devolving on him "his property, authority, and widow," is transformed into an arrangement by which, in the polygynic or monogamic family, the next brother loses by having to take steps for excluding himself from the succession.

The flaw in Mr. McLennan's argument appears to me to be this. He tacitly assumes that the succession of brothers to property, instead of sons, always implies the pre-existence of polyandry; whereas it merely implies the pre-existence of descent in the female line, which may or may not have had polyandry as a concomitant. There are hosts of cases where descent in the female line exists, and where there is neither polyandry now nor any sign of its past existence.

In the small space available, I must meet Mr. McLennan's rejoinders to my criticisms on his theory of primitive marriage, in the briefest manner. He sets forth his leading propositions thus: —

(1.) That "the form [of capture] represents and is a remainder of an actual system of capturing women for wives." As showing that the form does not necessarily imply capture from foreign tribes, I have pointed out that actual capture, and consequently the form of capture, may originate within the tribe; first, from the fighting of the men with one another for the possession of

women; second, from the resistance of the pursued women themselves, due to coyness, partly real and partly assumed; third, from the accompanying resistance of sympathizing women; and fourth, from the resistance of parents who are deprived of the services of daughters by their marriages. I have given numerous examples of acts of capture having such origins, and these Mr. McLennan passes over unnoticed.

(2.) That "a practice of capturing women for wives could not have become systematic unless it were developed and sustained by some rule of law or custom, which made it necessary as a means to marriage." This proposition implies that some "rule of law" was first established, in some way unspecified, and that capturing women became systematic as a consequence; which is not a solution of the problem but a postponement of it. The assumed pre-existence of such a law seems to me akin to the hypothesis of a primitive "social contract."

(3.) That "the rule of law or custom which had this effect was exogamy, the law (previously unnamed) which declared it incest for a man to marry a woman of the same blood or stock with himself." On which my comment, simply a more specific form of the last, is that we are thus required to conclude that the notions of "blood or stock" and of "incest" preceded the practice of stealing women; though this practice, found among the very lowest men, is a natural sequence of instincts which must have been in action before the earliest social groups were formed.

From these general rejoinders I pass to more special ones.

Mr. McLennan says:—"In this inquiry it was the existence of exogamy as an essential concomitant of capture that concerned me. I neither investigated nor had occasion to investigate its origin." Considering that the title of Mr. McLennan's work as originally published

was *Primitive Marriage: an Inquiry into the Origin of the Form of Capture in Marriage Ceremonies*, it seems strange that he should say he was not concerned with the explanation of exogamy. To ascribe capture to exogamy and to assign no cause for exogamy, is to give a very inadequate theory of primitive marriage. Mr. McLennan, however, while alleging that this problem did not concern him, says he threw out the suggestion that "practice of female infanticide" originated the correlative usages of capture and exogamy. I was quite unaware till now that Mr. McLennan laid so little stress upon this part of the theory. The title he gives to Chapter VII. of his work—"Exogamy: its Origin," &c., seems to imply that the explanation of it did concern him, though he now says it did not. In this chapter (pp. 110, 111, new edition), he assigns female infanticide as the cause, without any warning that this is to be taken merely as a suggestion. And to the growth of the consequent "usage induced by necessity" of stealing wives, he ascribes the "prejudice strong as a principle of religion..... against marrying women of their own stock,"—ascribes, that is, the law of exogamy. I have given several reasons for concluding that exogamy did not arise from this cause; and, as Mr. McLennan now states that what he said about this cause had "perhaps better have been left unsaid," I presume that he admits the validity of these reasons.

Mr. McLennan makes a counter criticism on the explanation of exogamy given by me. This explanation is that in warlike tribes, capturing of a foreign woman, implying conquest over enemies, was a mark of bravery and therefore honourable; that as a tribe became predominantly warlike, the honourableness of having a foreign wife became so relatively great, that taking a native wife became discreditable; and that finally, in the most warlike tribes, it became imperative that a wife should

be of foreign blood. Mr. McLennan objects that there is a gulf "between an act which is discreditable, and an act which is criminal."

"To me," he says, "it seems simply not possible to deduce from marriages with foreign women being deemed ever so honourable, that marriages with native women should be branded as incestuous—be deemed among the most impious of actions, and become capital offences."

My first reply is that though this "seems simply not possible" to Mr. McLennan, he might have found analogies which would show him its possibility. Is it not deemed honourable to conquer in war? Does it not become by consequence dishonourable to give way in battle and flee from the enemy? And are there not cases in which the dishonourableness of fleeing from the enemy became a penal offence, followed sometimes even by death? My second reply is that in the primitive state to which we must go back for the explanation of such practices as exogamy, no such notion as that of crime exists. Mr. McLennan's objection implies the belief that moral ideas antecede the earliest social state; whereas they are products of the social state, developing only as it advances. What we call crimes are thought creditable by many uncivilized men. Murder was no disgrace to a Fijian, but a glory; and his honour increased with the number of men he devoured. Among some tribes of the Pacific States, where the stronger man takes whatever he pleases from the weaker, the criminality of robbery is unrecognised. And by those many peoples whom I have instanced (*Prin. of Sociology*, § 281) as very commonly forming incestuous unions, incest is not regarded as criminal. How, then, can there be the impassable gulf Mr. McLennan supposes between the disgracefulness of marrying within the tribe and the crime of incest, when, originally, incest was not a crime?

By way of proof that among rude races a man does

not gain honour from a captured wife, Mr. McLennan gives some cases showing that captured wives are not themselves held in higher estimation than native wives, but in lower. I have neither said nor implied anything at variance with his facts. To assert the honourableness of capturing is not to assert the honourableness of being captured.

One objection raised by Mr. McLennan to the explanation I have given has a considerable appearance of validity, and some real validity; though it is an imprudent objection for him to make, since it tells against his own view more than against mine. He points out that if, in an extremely-warlike tribe, wiving with foreigners becomes imperative, and marriage with native-born women is disallowed, there arises the question, what becomes of the native-born women; and he says they must be "doomed to perpetual celibacy." In answer, I may point to the fact alleged by Mr. McLennan himself (*Studies*, &c., p. 112), that in some cases all the female children born within the tribe are destroyed, whence it follows that, in these cases at any rate, there results no such difficulty as that which he alleges. Further, I have to repeat the objection made by me to his hypothesis, that among a cluster of tribes practising primitive exogamy, as Mr. McLennan describes it, the female children born within each tribe not only become useless to the tribe, because unmarriageable by its members, but the rearing of them benefits and strengthens hostile tribes, who alone can utilize them: whence a motive to universal female infanticide throughout the tribes. But the truth to which Mr. McLennan's objection points, I take to be this; that, save in such extreme cases as the one I have cited above, exogamy, under that primitive form which implies actual capture of women from other tribes, does not become absolute; and that it acquires the character of a

peremptory law, only when the prevalence of women counted as foreign by blood within the tribe, introduces the secondary or derived form of exogamy, and makes obedience to the peremptory law practicable.

Mr. McLennan alleges that the explanation I have given could account "only for a limited practice of capturing women for wives", and that for this reason, "apparently," I have formed the opinion that exogamy is not normal but exceptional. I do not know why he says this; since the explanation I have given implies that everywhere, hostilities among tribes tend to produce exogamy in some and endogamy in others, and that thus the simultaneous genesis of the two is normal. If, however, by the words "that exogamy, properly so-called, was normal, is beyond dispute," he means that it was normal in the literal sense, as having originally been the rule and other practises exceptions—if he means again to express the belief he did originally, that exogamy has "been practised at a certain stage among every race of mankind"—if, by the additional instances of it which he now gives, he means to support this proposition; then I have simply to set against it the admission he makes (*Studies*, &c., p. 116) that exogamy and endogamy "may be equally archaic," and the statement that "the separate endogamous are nearly as numerous, and they are in some respects as rude, as the separate exogamous tribes" (*Ibid.*, p. 116)—an admission and a statement which harmonize perfectly with the hypothesis I have set forth, but are incongruous with Mr. McLennan's own hypothesis.

I have reserved to the last the most serious of Mr. McLennan's allegations against me, "That Mr. Spencer has failed to grasp the meaning of the terms exogamy and endogamy appears beyond dispute," he says. If this be true, the fault must be either in Mr. McLennan's statement of his views, or in my capacity for compre-

hension; and I suppose that in politeness I am bound to regard the fault as lying in me. I am reluctant, however, to leave the reader without the opportunity of forming his own judgment on this point; and I therefore lay before him the data as briefly as consists with clearness.

The question being how there arose the contrast between those tribes which married only with women of other tribes, or of foreign blood, and those tribes which married native women, the words "exogamy" and "endogamy," introduced by Mr. McLennan, were used by me as indicating these two systems, alike in their partially-established and in their completely-established forms. Employing the words in these unspecialized senses, I have referred to some societies as partially exogamous or partially endogamous, and have said that "exogamy and endogamy in many cases co-exist:" meaning, thereby, that in so far as the men of a tribe marry out of the tribe the tribe is exogamous, and in so far as they marry within the tribe the tribe is endogamous. This fact is cited by Mr. McLennan as "*proof* that the problem never was comprehended by" me. Giving to the words more special meanings than are necessitated by their literal significations, Mr. McLennan represents them as applicable only where marriage with women of the same stock is respectively forbidden or required. There cannot, consequently, be such things as partial exogamy or endogamy—the two are mutually exclusive. "The words," he says, "were not defined by me to denote practices at all, but *rules* or *laws*;" and he says that until there is actual prohibition of one or other, there is no law of marriage at all, and therefore no exogamy or endogamy.

Now Mr. McLennan may, of course, give what definitions he pleases to words introduced by himself. But I am at loss to understand how an evolutionist, which

Mr. McLennan declares himself to be, can ignore those antecedent stages that must have been passed through before exogamy and endogamy could become laws. Mr.

McLennan's familiarity with savage life must make him fully conscious that law, in our sense, is originally unknown; and that that genesis of laws out of customs which advanced societies show us, is implied by the state of the earliest societies in which no customs have yet evolved into laws. An evolutionist might be expected to regard it as a necessary implication that before exogamy and endogamy became laws they must have been practices.

If, instead of saying that I "never comprehended the meanings of the terms exogamy or endogamy," Mr. McLennan had said that I failed to comprehend how he reconciles his own uses of them with the meanings he gives, I should have agreed with him. On p. 230 in the chapter headed "Conclusion," (not, be it observed, in the chapter which he describes as "preliminary," and therefore only approximate in its statements) I find the following passage, in which I have italicised the significant words:

"On the whole, the account which we have given of the *origin of exogamy* appears the only one which will bear examination. The scarcity of women within the group led to a practice of stealing the women of other groups, and *in time it came to be considered improper*, because it was unusual for a man to marry a woman of his own group."

This passage, summing up the results of Mr. McLennan's inquiries, while it tacitly asserts that "the origin of exogamy" was a chief problem (though Mr. McLennan now says it did not concern him), applies the name exogamy to a *practice* that had not yet become a *law*. Even now, on the first page of the above article, he uses it in the same sense when he speaks of his original sug-

gestion thus—"the practice of capture somehow introducing exogamy, and exogamy thereafter perpetuating and extending the practice of capture." If, then, because I have applied the name exogamy to a growing custom that had not yet hardened into a law, I am charged with not understanding what exogamy means, I have simply to reply that the charge recoils with fatal effect on Mr. McLennan himself; since he uses the word in the same sense.

PROF. TAIT ON THE FORMULA OF
EVOLUTION.

It would be undesirable to give permanence to the subjoined communication, published in *Nature* for Dec. 2, 1880, were it not that it serves as a text for some remarks on scientific culture and the perverting influences caused by limitation of it to special sciences.

Initiated by a criticism of *First Principles* in the *British Quarterly Review* for October, 1873, there grew up a controversy carried on partly in pamphlets which I published and partly in the columns of *Nature*. In the course of it Prof. Tait, who, as a high authority, was quoted against me, became implicated and himself eventually entered the lists. Some time afterwards he uttered from his professorial chair at Edinburgh an address condemnatory of my views. It was published in *Nature* for Nov. 25, 1880, and drew from me the reply here reproduced.

Usually my polemical writings have, I believe, been considered as duly respectful of the feelings of antagonists. If an exception is here furnished, my excuse must be that I was, perhaps improperly, influenced by the example of Prof. Tait, who, repeating a comparison he made once before, told his students that—"When the purposely vague statements of the materialists and agnostics are thus stripped of the tinsel of highflown and unintelligible language, the eyes of the thoughtless who have accepted them on authority (!) are at last opened, and they are ready to exclaim with Titania 'Methinks I was enamour'd of an ass.'"

When, in *Nature* for July 17th, 1879, while reviewing Sir Edmund Beckett's book, Prof. Tait lugged in Mr. Kirkman's travesty of the definition of Evolution, most readers probably failed to see why he made this not very relevant quotation. But those who remembered a controversy which occurred some years previously, possibly divined the feeling which prompted him thus to go out of his way.

At the time I said nothing; but having recently had to prepare a new edition of *First Principles*, and thinking it well to take some notice of books, and parts of books, that have been written in refutation of that work, I decided to deal also with Mr. Kirkman's implied criticism, in which Prof. Tait so heartily concurred; and by way of gauging Prof. Tait's judgment on this matter, I thought it not amiss to give some samples of his judgment on matters falling within his own department. To make it accessible to those possessing previous editions of *First Principles*, the Appendix containing these replies to critics was published as a pamphlet.

In the inaugural lecture of this session, recently given to his students, part of which is published in the last number of *Nature*, Prof. Tait first of all recalls a passage from the preceding controversy. From this he quotes, or rather describes, a clause which, standing by itself, appears sufficiently absurd; and he marks the absurdity by a double note of admiration. Whether when taken with its context it is absurd, the reader will be able to judge on reading the passage to which it belongs.

In disproof of certain conclusions of mine, there had been quoted against me the *dictum* of Prof. Tait concerning the laws of motion, which is that—"as the properties of matter might have been such as to render a totally different set of laws axiomatic, these laws must be considered as resting on convictions drawn from observation and experiment and not on intuitive perception." Not urging minor objections to this *dictum*, I went on to say:—"It will suffice if I examine the nature of this proposition that 'the properties of matter *might have been*' other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments? Is it an intuition? If so, then along with doubt of an intuitive belief concerning things *as they are*, there goes confidence in an intuitive belief concerning things *as they are not*. Is it an hypothesis? If so, the implication is that a cognition of which the negation is inconceivable (for an axiom is such) may be discredited by inference from that which is not a cognition at all, but simply a supposition..... I shall take it as unquestionable that nothing concluded can have a warrant higher than that from which it is concluded, though it may have a lower. Now the elements of the proposition before us are these:—*As* 'the properties of matter might have been such as to render a totally different set of laws axiomatic' [*therefore*] 'these laws [now in force] must be considered as resting... not on

intuitive perception:’ that is, the intuitions in which these laws are recognised, must not be held authoritative. Here the cognition posited as premiss, is that the properties of matter might have been other than they are; and the conclusion is that our intuitions relative to existing properties are uncertain. Hence, if this conclusion is valid, it is valid because the cognition or intuition respecting what might have been, is more trustworthy than the cognition or intuition respecting what is!”

From which it is manifest that, when asking (of course ironically) whether this alleged truth was an experimentally-ascertained one, my purpose was partly to enumerate and test all imaginable suppositions respecting the nature of Prof. Tait’s proposition, and partly to show that he had affirmed something concerning the properties of matter which cannot be experimentally verified, and therefore which, by his own showing, he has no right to affirm.

The first example which, in my recent replies to criticisms, I have given of Prof. Tait’s way of thinking, is disclosed by a comparison of his views concerning our knowledge of the universe as visible to us, and our knowledge of an alleged invisible universe. This comparison shows that:—

“He thinks that while no validity can be claimed for our judgments respecting perceived forces, save as experimentally justified, some validity can be claimed for our judgments respecting unperceived forces, where no experimental justification is possible.”

Part of Prof. Tait’s answer is that “the theory there developed [in the *Unseen Universe*] was not put forward as probable, its purpose was attained when it was shown to be conceivable.” To which I rejoin that whereas Prof. Tait said he found in this theory a support for certain theological beliefs, he now confesses that he found none; for if no probability is alleged, no support

can be derived. The other part of his answer concerns the main issue. After pointing out that the argument of this work, "carried on in pursuance of physical laws established by converse with the universe we know, extends them to the universe we do not know," I had urged that if we have "no warrant for asserting a physical axiom save as a generalisation of results of experiments—if, consequently, where no observation or experiment is possible, reasoning after physical methods can have no place; then there can be no basis for any conclusion respecting the physical relations of the seen and the unseen universes," "since, by the definition of it, one term of the relation is absent." Prof. Tait's explanation is extremely startling. When following the discussion in the *Unseen Universe*, throughout which the law of the Conversation of Energy and the Principle of Continuity are extended from the tangible and visible matter and motion around us to an unknown form of existence with which they are supposed to be connected, readers little thought that Prof. Tait meant by this unknown form of existence his own mind. Yet this is all that he now names as the missing term of the relation between the seen universe and the unseen universe.

The second sample which I gave of Prof. Tait's views on matters pertaining to his own subject, concerned the nature of inertia, which he describes by implication as a positive force. Here I quoted Prof. Clerk Maxwell. To repeat his criticism in full would cause me to trespass on the pages of *Nature* even more unduly than I must do. If, however, any reader turns to *Nature*, July 3rd, 1879, and reads the passage in question, he will be able to judge whether it is, or is not, a joke, and if a joke, at whose expense. Meanwhile, the essential question remains. Prof. Tait says that matter has "an innate power of resisting external influences." I, contrariwise, say that the assertion of such a power is at variance with

established physical principles.

One further illustration of Prof. Tait's way of thinking was added. Quoting from a lecture given by him at Glasgow, for the purpose of dispelling "the widespread ignorance as to some of the most important elementary principles of physics," I compared two different definitions of force it contained. In a passage from Newton, emphatically approved by Prof. Tait, force is implied to be that which changes the state of a body, or, in modern language, does work upon it. Later on in the lecture, Prof. Tait says—"force is the rate at which an agent does work per unit of length." I contended that these definitions are irreconcilable with one another; and I do not see that Prof. Tait has done anything to reconcile them. True, he has given us some mathematics, by which he considers the reconciliation to be effected; and, possibly, some readers, awed by his equations, and forgetting that in symbolic operations, carried on no matter how rigorously, the worth of what comes out depends wholly on what is put in, will suppose that Prof. Tait must be right. If, however, his mathematics prove that while force is an agent which does work, it is also the rate at which an agent does work, then I say—so much the worse for his mathematics.

From these several tests of Prof. Tait's judgment, in respect to which I fail to see that he has disposed of my allegations, I pass now to his implied judgment on the formula, or definition, of Evolution. And here I have, first to ask him some questions. He says that because he has used the word "definition" instead of „formula," he has incurred my "sore displeasure and grave censure." In what place have I expressed or implied displeasure or censure in relation to this substitution of terms? Alleging that I have an obvious motive for calling it a "formula," he says I am "indignant at its being called a *definition*." I wish to see the words in which I have ex-

pressed my indignation; and shall be glad if Prof. Tait will quote them. He says—"It seems I should have called him the *discoverer of the formula!*" instead of "the inventor of the definition." Will he oblige me by pointing out where I have used either the one phrase or the other? These assertions of Prof. Tait are to me utterly incomprehensible. I have nowhere either said or implied any of the things which he here specifies. So far am I from consciously preferring one of these words to the other, that, until I read this passage in Prof. Tait's lecture, I did not even know that I was in the habit of saying "formula" rather than "definition." The whole of these statements are fictions, pure and absolute.

My intentional use of the one word rather than the other, is alleged by him *à propos* of an incidental comparison I have made. To a critic who had said that the formula or definition of Evolution "seems at best rather the blank form for a universe than anything corresponding to the actual world about us," I had replied that it might similarly be "remarked that the formula—'bodies attract one another directly as their masses and inversely as the squares of their distances,' was at best but a blank form for solar systems and sidereal clusters." Whereupon Prof. Tait assumes that I put the "Formula of Evolution alongside of the Law of Gravitation," in respect to the definiteness of the previsions they severally enable us to make; and he proceeds to twit me with inability to predict what will be the condition of Europe four years hence, as astronomers "predict the positions of known celestial bodies four years beforehand." Here we have another example of Prof. Tait's peculiarity of thought. Because two abstract generalisations are compared as both being utterly unlike the groups of concrete facts interpreted by them, *therefore* they are compared in respect to their other characters.

But now I am not unwilling to deal with the contrast

Prof. Tait draws; and am prepared to show that when the conditions are analogous, the contrast disappears. It seems strange that I should have to point out to a scientific man in his position, that an alleged law may be perfectly true, and that yet, where the elements of a problem to be dealt with under it are numerous, no specific deduction can be drawn. Does not Prof. Tait from time to time teach his students that in proportion as the number of factors concerned in the production of any phenomenon becomes great, and also in proportion as those factors admit of less exact measurement, any prediction made concerning the phenomenon becomes less definite; and that where the factors are multitudinous and not measurable, nothing but some general result can be foreseen, and often not even that? Prof. Tait ignores the fact that the positions of planets and satellites admit of definite prevision, only because the forces which appreciably affect them are few; and he ignores the fact that where further such forces, not easily measured, come into play, the previsions are imperfect and often wholly wrong, as in the case of comets; and he ignores the fact that where the number of bodies affecting one another by mutual gravitation is great, no definite prevision of their positions is possible. If Prof. Tait were living in one of the globular star-clusters, does he think that after observations duly taken, calculations based on the law of gravitation would enable him to predict the positions of the component stars four years hence? By an intelligence immeasurably transcending the human, with a mathematics to match, such prevision would doubtless be possible; but considered from the human standpoint, the law of gravitation even when uncomplicated by other laws, can yield under such conditions only general and not special results. And if Prof. Tait will deign to look into *First Principles*, which he apparently prides himself on not having done, he will there

find a sufficient number of illustrations showing that not only orders of changes, but even social changes, are predictable in respect to their general, if not in respect to their special, characters.

There remains only to notice the opinion which Prof. Tait seems still to hold, that the verbal transformation which Mr. Kirkman has made in the formula or definition of Evolution, suffices to show its hollowness. Here I may be excused for repeating what I have already said elsewhere, namely, that "We may conveniently observe the nature of Mr. Kirkman's belief, by listening to an imaginary addition to that address before the Literary and Philosophical Society of Liverpool, in which he first set forth the leading ideas of his volume; and we may fitly, in this imaginary addition, adopt the manner in which he delights.

"Observe, gentlemen," we may suppose him saying, "I have here the yolk of an egg. The evolutionists, using their jargon, say that one of its characters is 'homogeneity'; and if you do not examine your thoughts, perhaps you may think that the word conveys some idea. But now if I translate it into plain English, and say that one of the characters of his yolk is 'all-alikeness,' you at once perceive how nonsensical is their statement. You see that the substance of the yolk is not all-alike, and that therefore all-alikeness cannot be one of its attributes. Similarly with the other pretentious term 'heterogeneity,' which, according to them, describes the state things are brought to by what they call evolution. It is mere empty sound, as is manifest if I do but transform it, as I did the other, and say instead 'not-all-alikeness.' For on showing you this chick into which the yolk of the egg turns, you will see that 'not-all-alikeness' is a character which cannot be claimed for it. How can any one say that the parts of the chick are not-all-alike? Again, in their blatant language we are told that evolu-

tion is carried on by continuous 'differentiations'; and they would have us believe that this word expresses some fact. But if we put instead of it 'somethingelseifications' the delusion they try to practise on us becomes clear. How can they say that while the parts have been forming themselves the heart has been becoming something else than the stomach, and the leg something else than the wing, and the head something else than the tail? The like manifestly happens when for 'integrations' we read 'sticktogetherations'; what sense the term might seem to have, becomes obvious nonsense when the substituted word is used. For nobody dares assert that the parts of the chick stick together any more than do the parts of the yolk. I need hardly show you that now when I take a portion of the yolk between my fingers and pull, and now when I take any part of the chick, as the leg, and pull, the first resists just as much as the last—the last does not stick together any more than the first; so that there has been no progress in 'sticktogetherations.' And thus, gentlemen, you perceive that these big words which, to the disgrace of the Royal Society, appear even in papers published by it, are mere empty bladders which these would-be philosophers use to buoy up their ridiculous doctrines."

But though it is here, I think, made apparent enough that even when disguised in Mr. Kirkman's grotesque words, the definition of Evolution continues truly to express the facts, Prof. Tait shows no sign of changing his original opinion that Mr. Kirkman has made "an exquisite translation" of the definition. Nay, so charmed does he appear to be with Mr. Kirkman's feats of this nature, that he gives us another of them. One of two conclusions must be drawn. Prof. Tait either thinks that fallacies are disclosed by the aid of these cacophonous long words, or else the clatter of curious syllabic compounds greatly excites his sense of humour. In the

last case we may infer that had he been one of that "Twelfth Night" party in which the Clown exclaims—"I did impetico thy gratillity," he would have joined in Sir Andrew Aguecheek's applause.

In his essay on "The Study of Mathematics as an Exercise of the Mind," Sir William Hamilton insisted with great force upon the unfitness of mathematically-disciplined men for contingent reasoning: giving proof that "a too exclusive study of these sciences is, absolutely, to disqualify the mind for observation and common reasoning." In support of this thesis, he marshalled numerous high authorities, including, along with various distinguished non-mathematicians, the mathematicians themselves—Pascal, Descartes, D'Alembert, and others. To earlier examples of mental defects produced, which might be given, a conspicuous addition has been supplied recently: that furnished by M. Michel Chasles, who, in the matter of the Newton-Pascal forgeries, surprised both the scientific world and the world at large by his extreme inability to judge of evidence and detect imposture. Personal experience has yielded verification. Observation of one much devoted to geometry forced on me the conclusion that a prevailing fault in general reasoning had been produced in him. Such a result is not to be wondered at. The mathematician does not deal with many indefinite data, but with a few definite ones. In his operations there occurs no collecting of evidence: his successive inferences are inevitable implications. Ba-

lancing of probabilities is never thought of: his deductions are necessary and unqualified. The mode of thought generated affects his reasoning about other matters than the mathematical and the mathematico-physical. Assuming simplicity and definiteness of data where these do not exist, he draws conclusions which, as being drawn mathematically, he thinks unquestionable. A distinguished mathematician and physicist now living has more than once illustrated this truth.

A further mental effect is produced. The habit of dealing with conclusions from data that are few and exact, appears to entail an inability to recognize the conclusions drawn from inexact and complex data as constituting parts of scientific knowledge. In the minds of those thus characterized science exists as a multitude of separate demonstrated propositions; and it never occurs to them that in the order of Nature these must be parts of a whole. The merging of them in some universal truth is an idea so alien that the very terms required seem meaningless, and the man who uses them a charlatan. If, referring to an architect, a mason should say—"He a builder! Why he never dressed a stone in his life!" he would betray a feeling not altogether dissimilar. Already in the appendix to *First Principles* above referred to, I have pointed out how some men of letters and some mathematicians, alike in having minds insufficiently supplied with the materials out of which the conception of Evolution is to be framed, regard the

definition of Evolution as a combination of empty words: Prof. Tait and Mr. Kirkman being named in illustration. And I ought to have there added the illustration furnished by a Senior Wrangler who reviewed *First Principles* in the *British Quarterly Review* for October 1873, and with whom I subsequently carried on a controversy.

Since then two further illustrations have come to my knowledge. One is contained in the Life of the late Dr. Romanes. Writing to Mr. Darwin in 1880, and returning some letters, he says:—

“The latter convey exactly the criticism that I should have expected from—, for while writing my essay on Theism I had several conversations with him upon the subject of Spencer’s writings, and so know exactly what he thinks of them. But in none of these conversations could I get at anything more definite than is conveyed by the returned letters. In no point of any importance did he make it clear to me that Spencer was wrong, and the only result of our conversation was to show me that in —’s opinion it was only my ignorance of mathematics that prevented me from seeing that Mr. Spencer is merely a ‘word philosopher’...” (pp. 95—6).

The other illustration, of somewhat earlier date, will be found in the *Edinburgh Review* for January 1884. The writer of it was among the wranglers of his year. His characterization of *First Principles* runs as follows:—
 “This is nothing but a *philosophy of epithets and phrases*, introduced and carried on with an unrivalled solemnity and affection of precision of style, concealing

the loosest reasoning and the haziest indefiniteness.”*

This instancing of five men, occupied with mathematics and mathematical physics, in whose minds the formula of Evolution raised no answering conception, may be thought to imply an undervaluation, if not even a reprobation, of mathematics and physics as subjects of study. No inference could be more erroneous. To guard against it, however, let me point out that while exclusive devotion to the exact sciences produces certain defects of thought, exclusive devotion to the inexact sciences produces defects of thought of an opposite kind. These last present phenomena under such complex forms, with interdependencies so involved, that necessities of relation cannot in most cases be said to exist; and the many causes simultaneously in operation so obscure the action of any one, as in large measure to exclude the idea of definite causation. Among plants a few fundamental relations may be fairly alleged, as between the monocotyledonous germination and the endogenous mode of growth, or between the dicotyledonous germination and the exogenous mode of growth. But relations among multitudinous combined traits, such as kind of fructification and possession of thorns, or hard-shelled nuts and shapes of leaves, cannot be shown to have any causal

*) Some amusement was caused by the mode in which I dealt with this sweeping condemnation. It appeared just before the 6th edition of *First Principles* was issued. In pursuance of my directions, Messrs Williams and Norgate, when sending out advertisements of this new edition, appended to each of them the above sentences, as expressing the opinion of the *Edinburgh Review*. The advertisements were published in all the leading daily and weekly papers.

characters. So with animals. Though it is a trait of creatures having mammæ to have seven cervical vertebræ, yet for this correlation of structures no necessity can be alleged; as is proved by the fact that though at one time the connexion was supposed to be universal, there have of late years been discovered mammals having eight vertebræ in the neck. Hence, those who exclusively study animals and plants, being perpetually impressed by connexions of facts which are either fortuitous or for which no reason can be assigned, are not daily habituated to the perception of causal relations, and such generalizations as they can establish come to be regarded as empirical. A purely inductive habit is encouraged and a deductive habit discouraged. The resulting mental tendencies operate in other regions of thought, so that everywhere necessity of relation is doubted, and the idea of inevitable consequence meets with no acceptance. Many times in a distinguished biologist I have observed the effect thus described. Present him with a great accumulation of evidence supporting a certain conclusion, and this conclusion, coming before him under the form of an induction, he would entertain and seem ready to accept. After a time point out that this conclusion might be reached deductively from known necessary truths, and immediately his scepticism was aroused. Forgetting the inductive basis originally assigned, the deductive proof excited such repugnance as tended to make him reject what he before admitted. The habit

of mind encouraged by dealing exclusively with empirical generalizations produced an abnormal distrust of all others.

Is it then that ability to form balanced judgments about things at large demands discipline in all the sciences? The answer is Yes and No. And here presents itself a question often raised and never settled—Is it better to have an extensive or fairly complete knowledge of a single science, or a general acquaintance with all the sciences? The tacit implication is that the choice is between restriction with accuracy and breadth with superficiality. But this is not true. The error lies in supposing that a general knowledge is the same thing as a superficial knowledge. There may be full comprehension of the essentials of a science without familiarity with its details—a clear understanding of those fundamental truths from which all the multitudinous minor truths constituting it are deductions. Take the case of mechanics. In a moderate time a student may master its cardinal ideas—the composition and resolution of forces; the general principle of inertia; the laws of motion, including acceleration and retardation and the various compoundings of motions, studied in connexion with the conservation of energy; the doctrine of stable and unstable equilibrium, with the relations of statics and dynamics; and may add to these the theorems concerning the mechanical powers. The abstract truths comprehended under these heads having been severally brought home

in connexion with some concrete applications, an adequate grasp of mechanical principles is obtained, which, though only general, is not superficial; and which gives the power intelligently to appreciate the higher and more complex conclusions of the science when upon occasion they are presented. Kindred courses may similarly bring within the students clear comprehension the fundamentals of all the sciences; and he will then be in a condition for devoting himself efficiently to the science he prefers. Alike for the sake of knowledge and for the sake of discipline the ideal course of culture is—the ground truths of each science joined with mastery of one.

ABILITY *VERSUS* INFORMATION.

Among my papers I find in print the following letter written to Dr. (now Sir) Henry Acland. Under what circumstances it was written I do not know; nor can I remember in what shape it was published. Probably it formed part of some collection of opinions, respecting University Education, issued in 1882: the date of it being March 4 of that year. I give it a place here as expressing a strong conviction of mine concerning the quality of ordinary intellectual culture.

I am just now allowing myself, very imprudently, to be drawn away from my usual line of work, and am therefore the less able to consider at length the matter

to which your letter of the 2nd draws my attention. Moreover, I feel that even had I any amount of energy to spare, my opinion upon the details of the proposed forms of examination would not be of much value.

There is only one general criticism which I feel inclined to make upon the examination papers you have forwarded—a criticism to which I think they are open in common with examination papers at large. They are drawn up with the exclusive view of testing *acquisition* rather than *power*. I hold that the more important thing to be ascertained by an examination is not the quantity of knowledge which a man has taken in and is able to pour out again, but the ability he shows to use the knowledge he has acquired; and I think that examinations of all kinds are habitually faulty, inasmuch as they use the first test rather than the last, by which to judge of superiority.

I hold that in every examination there should be a certain set of questions devised for the purpose of ascertaining what capacity for original thinking the candidate has—questions to which he will find no answer in the books that he has read, but to which answers must be elaborated by himself from reflection upon the knowledge he has acquired. To give an example of what I mean, there might be put to biological students in the physiological part of their examination such a question as—What are the other characteristics of the Aloe which are related to the long delay in its flowering, and which make this delay profitable to the species? If some few questions of this kind, for which the student was wholly unprepared, were included in every examination, they would serve to single out the few men who were something more than mere passive recipients of book knowledge and professorial teaching.

BOOK-DISTRIBUTION.

When the late Mr. Fawcett was Postmaster-General I wrote to him a letter suggesting a system which would, it seemed to me, greatly facilitate (and therefore cheapen) the process of conveying books from publishers to readers. Nothing came of my proposal: the existing facilities were held sufficient. I think it well, however, to give permanence to the suggestion, hoping that some future Postmaster-General may take a different view. The date of the letter was June 5, 1882.

Thanks for your note some time since received, and for the copy of the *Postal Guide* drawing my attention to the postal order system. The unsatisfactoriness of this for the purpose I have in view is both that it involves a small tax and entails a considerable amount of clerical labour.

Within these last few days I have hit upon a device which, it seems to me, solves the problem satisfactorily; and on discussing the matter with Dr. William Smith, whose wide experience as a publisher of dictionaries makes him a good judge of the commerce of literature, he agrees with me that it is practicable, and that immense benefits might be achieved by adopting it. I inclose a postcard showing the plan I propose. It is a supposed order for a book of my own. On the face it is, of course, addressed to the publisher. On the back the purchaser who wishes to have the book sent to him writes, as shown, the order, with his name and address. Below he affixes postage-stamps to the amount of the price: the space allowed being ample for the great majority of books if shilling stamps are used. He then scribbles over the affixed stamps so as to erase them and make them unavailable

even should they be detached. All these acts he may, if he please, go through at the post-office where he may purchase both the postcard and the stamps; and write what he has to say at the counter where postal-orders, etc. are drawn. He then posts the card; and it goes along with other letters and cards, and is delivered to the publisher of the book. The publisher addresses the required book to him (and were the system established he would have a whole stock of copies wrapped up and stamped ready for addressing); and he does the like with numerous other orders for other books. One of his clerks then takes the postcards, perhaps 50 to 100, received that morning, writes down from each the amount it bears in stamps, and adds up the column of all these values, ascertaining the total due from the post-office. The clerk then takes the publisher's stamp, bearing the name of the firm, and impresses it on each of the cards, showing that it has passed through the publisher's establishment. Then at the same time that he sends his messenger with the books to the post-office, the publisher also sends all these cards and list of their values to be handed in to the post-office clerk, who checks the list by the cards and having also checked the addition, gives it to some superior official to write out a cheque for the amount payable to the publisher.

The advantages achieved are these:—

1st. The book-purchaser, even if he goes himself to the post-office, has to go through no appreciably greater trouble than if he went to his retail bookseller and ordered the book; and if he has in his house post-cards and stamps to the amount required, less trouble is entailed upon him; since he may send the postcard by his servant along with other letters to the post-office.

2nd. Neither by the servant of the purchaser, nor by a post-office clerk, nor by a letter-carrier can the stamps sent in payment be utilized, even if they could be de-

tached by steaming or otherwise in such a manner as to be undamaged; for, being erased, they would be unavailable by anybody else. Being erased in the way shown they are of value only to the publisher to whom the card is addressed.

3rd. At the post-office where the card is posted no more trouble is entailed by it than by an ordinary letter; and the profit of the post-office in sending the order is provided for as it is in an ordinary letter or card.

4th. As the postcard thus bearing these stamps has to be stamped by the publisher to whom it is addressed before it can be exchanged at the post-office for its value, it is rendered unavailable by anyone else into whose hands it goes. The letter-carrier, even if dishonest, can make no use of it, seeing that he cannot get the use of the publisher's stamp. Only by forging the publisher's stamp would the card be rendered available by him; and then the danger of detection would be so great that the thing could not be done. In the first place, the publisher's ordinary messenger being known at the post-office, the presentation of cards by anyone else would at once draw attention; and, in the second place, any such considerable abstraction of cards from those which daily came to the publisher as would be required to make the robbery worth while, would at once raise inquiry; since there would come in a few days letters to the publisher from those who had not received the books ordered, and a hue and cry would be raised. Forging the publisher's stamp, which could be the only mode of theft, would thus be extremely rare, if it ever occurred.

5th. Moreover, since the post-office would pay the publisher by cheque, which might be made payable to order, no person of the class of a letter-carrier or a post-office clerk would be able, if he did commit forgery, to get the money.

6th. The only work which this process of transmission

would give to the post-office would be that of going through the number of postcards brought by the publisher's messenger, checking the list of the sums contained on them, and checking the addition. If this labour is divided over the whole number of cards, say from 50 to 100 brought at once, the amount of labour entailed by the transaction which each card represents is seen to be extremely small. To which add that against this amount of trouble given to the post-office there would be a countervailing economy. Under such a system the number of postcards delivered by each post to a publisher would be great; and since the chief cost of the postal system is in the delivery, the cost of delivery, when a great number of cards were taken at once by a letter-carrier to the publisher, would be, for each one of them, greatly reduced. Obviously, the diminished cost of delivery for each card would more than compensate for the amount of trouble taken in checking and adding up the amounts. If a halfpenny for a postcard suffices to pay for the cost of delivery at present, then there would actually be, by multiplication of transactions, a profit, rather than a loss to the post-office.

7th. Further, it is to be borne in mind that the post-office would make its profit on the postage of the books ordered. If the amounts now charged for the transmission of books are adequate to cover the cost and leave a margin, then whatever multiplies such transactions profits the post-office; and therefore, if there were any unpaid trouble, entailed by these cards upon the post-office, it would be repaid by the profits on the books sent according to order.

8th. Should it, however, be held that the transaction must be made to bring positive, obvious profit, then this end might easily be achieved by the requirement that a halfpenny postage-stamp should be affixed on the post-card in addition to the printed stamp.

My anxiety to get some such system adopted is due to the fact, which I think I indicated when I had the pleasure of talking with you at Mr. Eustace Smith's, that an immense lowering of price in books might be achieved in this way, and a consequent immense extension in their diffusion. The present system of distribution through wholesale houses and retail booksellers is an absurd anachronism. It grew up under, and was appropriate to, the ancient system of communication by coaches and subsequently by railway parcels; but is altogether inappropriate to a time when the book-post furnishes a better system of distribution. The survival of the old system is due to organized trade interests; and it immensely impedes the diffusion of books by paying for a labour which has become unnecessary. It is true that of late times the nominal prices of books are practically reduced by the discount of 2d. in the shilling, or even more by some retailers; but, in the first place, this does not prevent book-buyers from being often deterred from buying by reading in an advertisement the nominal price of a book, which they think too high to be afforded by them; and, in the second place, the reduction which these retailers make is nothing like as great as might be made if the labour of both wholesaler and retailer were done away with, as it ought now to be. Fully 40 per cent. of the published price of every book now goes to cover the cost of portorage—the cost of transferring the book from the publisher to the reader. This 40 per cent. by no means represents the entire enhancement of the published price of the book. Prices of books would be lowered by much more than 40 per cent. if this existing system could be replaced in the way I have described. As you know, better than I do, it is a familiar truth, especially to economists, that any tax on a commodity raises its price by more than the amount of the tax; and this holds very obviously in the present case. Let the

40 per cent. be deducted from the advertized prices of books, and immediately the demand for them becomes immensely greater, probably double. The demand being doubled makes it possible to obtain an adequate return with a smaller profit on each copy to author and publisher; and therefore prompts a still further reduction in the price, and this again a still further distribution, acting and re-acting. So that I do not doubt that the prices of books would, by the adoption of this system, be lowered by one half. As a further reason for this I should add that even the publisher could afford, setting aside the increase in his business, to lower his rate of profit on each copy; for the reason that his transactions would be much less costly. At present his business with wholesale and some retail houses entails a considerable amount of book-keeping, and a staff of clerks adapted to the labour. But in the system described the greater part of this book-keeping would disappear; the work of fulfilling the orders received would be purely mechanical; and a small staff of a lower capacity would suffice his needs, enabling him to diminish the rate of profit per copy he at present requires. Further, being prepaid, he would have to make no allowance for bad debts; and this would again diminish the needful rate of profit.

I may add that the great lowering in the price of books which would inevitably take place, would more especially tell upon the graver and higher priced books, which are now beyond the reach of the great mass of book-buyers.

M. DE LAVELEYE'S ERROR.

By way of introduction to this article nothing more is needed than to say that it was published in *The Con-*

temporary Review for April, 1885 under the title "A Rejoinder to M. de Laveleye." The misinterpretation he made of my political views is one very generally made, and these pages, which seek to exclude it, may therefore fitly have a permanent place.

The editor of the *Contemporary Review* having kindly allowed me to see a proof of the foregoing article by M. de Laveleye, and having assented to my request that I might be allowed to append a few explanations and comments, in place of a more formal reply in a future number of the *Review*, I have, in the following pages, set down as much as seems useful to prevent the grave misunderstandings likely to be produced by M. de Laveleye's criticisms, if they are permitted to pass unnoticed.

On the first page of his essay, M. de Laveleye, referring to the effort to establish "greater equality among men" by "appropriating State, or communal, revenues" for that end, writes—

"Mr. Spencer considers that this effort for the improvement of the condition of the working-classes, which is being everywhere made with greater or less energy, is a violation of natural laws, which will not fail to bring its own punishment on nations, thus misguided by a blind philanthropy" (p. 485).

As this sentence stands, and especially as joined with all which follows, it is calculated to produce the impression that I am opposed to measures "for the improvement of the condition of the working-classes." This is quite untrue, as numerous passages from my books would show. Two questions are involved—What are the measures? and—What is the agency for carrying them out? In the first place, there are various measures conducive to "improvement of the condition of the working-classes" which I have always contended, and still contend, de-

volve on public agencies, general and local—above all, an efficient administration of justice, by which they benefit both directly and indirectly—an administration such as not simply represses violence and fraud, but promptly brings down a penalty on every one who trespasses against his neighbour, even by a nuisance. While contending for the diminution of State-action of the positively-regulative kind, I have contended for the increase of State-action of the negatively-regulative kind—that kind which restrains the activities of citizens within the limits imposed by the existence of other citizens who have like claims to carry on their activities. I have shown that “maladministration of justice raises, very considerably, the cost of living for all;”* and is, therefore, felt especially by the working-classes, whose state is most closely dependent on the cost of living. As one of the evils of over-legislation, I have, from the beginning, urged that, while multitudinous other questions absorb public attention, the justice-question gets scarcely any attention; and social life is everywhere vitiated by the consequent inequities.† While defending *laissez-faire* in its original and proper sense, I have pointed out that the policy of universal meddling has for its concomitant that vicious *laissez-faire* which leaves dishonesty to flourish at the expense of honesty.§ In the second place, there are numerous other measures conducive to “the improvement of the condition of the working-classes” which I desire quite as much as M. de Laveleye to see undertaken; and simply differ from him concerning the agency by which they shall be undertaken. Without wishing to restrain philanthropic action, but quite contrariwise, I have in various places argued that philanthropy will better achieve its ends by non-government-

*) *Study of Sociology*, p. 415, postscript in library edition.

†) See *Social Statics*: “The Duty of the State.” Also *Essays*, vol. ii. pp. 94–8; vol. iii. p. 167.

§) *Study of Sociology*, pp. 351–3, cheap edition.

al means than by governmental means.* M. de Laveleye is much more familiar than I am with the facts showing that, in societies at large, the organized arrangements which carry on production and distribution have been evolved not only without State-help, but very generally in spite of State-hindrance; and hence I am surprised that he apparently gives no credence to the doctrine that, by private persons acting either individually or in combination, there may be better achieved multitudinous ends which it is the fashion to invoke State-agency for.

Speaking of the domain of individual liberty, M. de Laveleye says—

“To be brief, I agree with Mr. Herbert Spencer that, contrary to Rousseau’s doctrine, State power ought to be limited, and that a domain should be reserved to individual liberty which should be always respected; but the limits of this domain should be fixed, not by the people, but by reason and science, keeping in view what is best for the public welfare” (p. 488).

I am a good deal perplexed at finding the last clause of this sentence apparently addressed to me as though in opposition. *Social Statics* is a work mainly occupied with the endeavour to establish these limits by “reason and science.” In the *Data of Ethics*, I have sought, in a chapter entitled the “Sociological View,” to show how certain limits to individual liberty are deducible from the laws of life as carried on under social conditions. And in *The Man versus The State*, which M. de Laveleye is more particularly dealing with, one part of the last chapter is devoted to showing, deductively, the derivation of what are called “natural rights” from the vital needs which each man has to satisfy by activities pursued in presence of other men who have to satisfy like needs; while another part of the chapter is devoted to showing, inductively, how recognition of natural rights began, in the earliest social groups, to be initiated by

*) *Social Statics*: “Poor Laws.”

those retaliations which trespasses called forth—retaliations ever tending to produce respect for the proper limits of action. If M. de Laveleye does not consider this to be an establishment of limits “by reason and science,” what are the kinds of “reason and science” by which he expects to establish them?

On another page M. de Laveleye says—

“I am of opinion that the State should make use of its legitimate powers of action for the establishment of greater equality among men, in proportion to their personal merits” (p. 489).

Merely observing that the expression “its legitimate powers of action” seems to imply a begging of the question, since the chief point in dispute is—What are “its legitimate powers of action?” I go on to express my surprise at such a sentence coming from a distinguished political economist. M. de Laveleye refers to the “old-fashioned political economy,” implying that he is one of those younger economists who dissent from its doctrine; but I was quite unprepared to find that his dissent went so far as tacitly to deny that in the average of cases a proportioning of rewards to personal merits naturally takes place under the free play of supply and demand. Still less, after all the exposures made of the miseries inflicted on men throughout the past by the blundering attempts of the State to adjust prices and wages, did I expect to see in a political economist such a revived confidence in the State as would commission it to adjust men’s rewards “in proportion to their personal merits.” I hear that there are some who contend that payment should be proportionate to the disagreeableness of the work done: the implication, I suppose, being that the knacker and the nightman should receive two or three guineas a day, while a physician’s fee should be half-a-crown. But, with such a proportioning, I suspect that, as there would be no returns, adequate to repay the cost and time and la-

bour of preparation for the practice of medicine, physicians would quickly disappear; as would, indeed, all those required for the higher social functions. I do not suppose that M. de Laveleye contemplates a proportioning just of this kind. But if in face of all experience, past and present, he trusts officialism to judge of "personal merits," he is sanguine to a degree which surprises me.

One of the questions which M. de Laveleye asks is—
"If the intervention of public power for the improvement of the condition of the working-classes be a contradiction of history, and a return to ancient militant society, how is it that the country in which the new industrial organization is the most developed—that is to say, England—is also the country where State intervention is the most rapidly increasing, and where opinion is at the same time pressing for these powers of interference to be still further extended?" (p. 491).

Several questions are here raised besides the chief one. I have already pointed out that my objection is not to "intervention of public power for the improvement of the condition of the working-classes," but to interventions of certain kinds. The abolition of laws forbidding trade-combinations, and of laws forbidding the travelling of artisans, were surely measures which improved "the condition of the working-classes;" and these were measures which I should have been eager to join in obtaining. Similarly, at the present time I am desirous of seeing provided the easiest and most efficient remedies for sailors fraudulently betrayed into unseaworthy ships; and I heartily sympathize with those who denounce the continual encroachments of landowners—enclosures of commons and the turf-covered borders of lanes, &c. These, and kindred injustices to the working-classes, stretching far back, I am no less desirous to see remedied than is M. de Laveleye; provided always that due care is taken that other injustices are not committed in remedying

them. Evidently, then, this expression of M. de Laveleye raises a false issue. Again, he says that I call this public intervention on behalf of the working-classes "a return to ancient militant society." This is quite a mistake. In ancient militant society the condition of the working-classes was very little cared for, and, indeed, scarcely thought of. My assertion was that the *coercive system* employed was like the coercive system employed in a militant society: the *ends* to which the systems are directed being quite different. But turning to the chief point in his question, I meet it by counter-questions—Why is it that the "new industrial organization" is best developed in England? and—Under what conditions was it developed? I need hardly point out to M. de Laveleye that the period during which industrial organization in England developed more rapidly and extensively than elsewhere, was a period during which the form of government was less coercive than elsewhere, and the individual less interfered with than elsewhere. And if now, led by the admirers of Continental bureaucracies, eager philanthropists are more rapidly extending State-administrations here than they are being extended abroad, it is obviously because there is great scope for the further extension of them here, while abroad there is little scope for the further extension of them.

In justification of coercive methods for "improving the condition of the working-classes," M. de Laveleye says—

"One fact is sufficient to show the great progress due to this State legislation: in an ever-increasing population, crime is rapidly and greatly diminishing" (p. 496). Now, without dwelling on the fact, shown in Mr. Pike's *History of Crime in England*, that "violence and lawlessness" had increased during the war period which ended at Waterloo; and without dwelling on the fact that, after the recovery from prostration produced by war,

there was a diminution of crime along with that great diminution of coercive legislation which characterized the long period of peace; I go on to remark that a primary condition to the correct drawing of inferences is—other things equal. Does M. de Laveleye really think, when comparing the state of the last generation with that of the present, that other things are so equal that to the growth of State-administrations can be ascribed the decrease of crime? He ignores those two factors, far more important than all others, which have produced a social revolution—railways and free-trade: the last resulting from the abolition of governmental restraints after a long struggle, and the first effected by private enterprise carried out in spite of strenuous opposition for some time made in the Legislature. Beyond all question, the prosperity due to these factors has greatly ameliorated the condition of the working-classes, and by so doing has diminished crime; for undoubtedly, diminishing the difficulties of getting food, diminishes one of the temptations to crime. If M. de Laveleye refers to a more recent diminution, then, unless he denies the alleged relation between drunkenness and crime, he must admit that the temperance agitation, with its pledges, its “Bands of Hope,” and its “Blue Ribbon League,” has had a good deal to do with it.

Before passing to the chief question let me correct M. de Laveleye on some minor points. He says—

“I think that the great fundamental error of Mr. Herbert Spencer’s system, which is so generally accepted at the present day, consists in the belief that if State power were but sufficiently reduced,” &c.

Now I set against this a sentence not long since published by Mr. Frederic Harrison:

“Mr. Spencer has himself just published..... *The Man versus The State*, to which he hardly expects to make a convert except here and there, and about which an un-

friendly critic might say that it might be entitled 'Mr. Spencer against all England.''' (*Nineteenth Century*, vol. xvi. p. 366.)

The fear lest my arguments should prevail, which I presume prompted M. de Laveleye's article, is evidently ill-founded. I wish I saw reason to believe that his estimate is nearer to the truth than the opposite one.

On p. 490, M. de Laveleye writes—

"The law that Mr. Herbert Spencer desires society to adopt is simply Darwin's law—'the survival of the fittest.'"

Perhaps I may be excused for wishing here to prevent further confirmation of a current error. In his article, M. de Laveleye has quoted from *Social Statics* passages showing insistence on the benefits resulting from survival of the fittest among mankind, as well as among animals; though he ignores the fact that the work as a whole is an elaborate statement of the conditions under which, and limits within which, the natural process of elimination of the unfit should be allowed to operate. Here my immediate purpose is to correct the impression which his statement, as above worded, produces, by naming the dates: *Social Statics* was published in 1851; Mr. Darwin's *Origin of Species* in 1859.

And now I pass to the main issue. In pursuance of his statement that I wish society to adopt the survival of the fittest as its guiding principle, M. de Laveleye goes on to describe what would be its action as applied to mankind. Here are his words.

"This is the ideal order of things which, we are told, ought to prevail in human societies, but everything in our present organization (which economists, and even Mr. Spencer himself, admit, however, to be natural) is wholly opposed to any such conditions. An old and sickly lion captures a gazelle; his younger and stronger brother arrives, snatches away his prize, and lives to perpetuate

the species; the old one dies in the struggle, or is starved to death. Such is the beneficent law of the 'survival of the fittest.' It was thus among barbarian tribes. But could such a law exist in our present social order? Certainly not! The rich man, feebly constituted and sickly, protected by the law, enjoys his wealth, marries and has offspring, and if an Apollo of herculean strength attempted to take from him his possessions, or his wife, he would be thrown into prison, and were he to attempt to practise the Darwinian law of selection, he would certainly run a fair risk of the gallows" (p. 492).

Now though, on the next page, M. de Laveleye recognizes the fact that the survival of the fittest, as I construe it in its social applications, is the survival of the industrially superior and those who are fittest for the requirements of social life, yet, in the paragraph I have quoted, he implies that the view I hold would countenance violent methods of replacing the inferior by the superior. Unless he desires to suggest that I wish to see the principle operate among men as it operates among brutes, why did he write this paragraph? In the work before him, without referring to other works, he has abundant proof that, above all things, aggression of every kind is hateful to me; and he scarcely needs telling that from my earliest book, written more than a third of a century ago, down to the present time, I have urged the change of all laws which either inflict injustice or fail to remedy injustice, whether committed by one individual against another, or by class against class, or by people against people. Why, then, did M. de Laveleye make it seem that I would, if I could, establish a reign of injustice under its most brutal form? If there needs proof that in my view the struggle for existence as carried on in society, and the greater multiplication of those best fitted for the struggle, must be subject to rigorous limitations, I may quote as sufficient proof a passage from the *Data of Ethics*: premising that

the word cooperation used in it, must be understood in its widest sense, as comprehending all those combined activities by which citizens carry on social life.

“The leading traits of a code under which complete living through voluntary co-operation [here antithetically opposed to compulsory co-operation, characterizing the militant type of society] is secured, may be simply stated. The fundamental requirement is that the life-sustaining actions of each shall severally bring him the amounts and kinds of advantage naturally achieved by them; and this implies, firstly, that he shall suffer no direct aggressions on his person or property, and, secondly, that he shall suffer no indirect aggressions by breach of contract. Observance of these negative conditions to voluntary co-operation having facilitated life to the greatest extent by exchange of services under agreement, life is to be further facilitated by exchange of services beyond agreement: the highest life being reached only when, besides helping to complete one another's lives by specified reciprocities of aid, men otherwise help to complete one another's lives” (p. 149).

This passage, indeed, raises in a convenient form the essential question. It will be observed that in it are specified two sets of conditions, by conforming to which men living together may achieve the greatest happiness. The first set of conditions is that which we comprehend under the general name *justice*; the second set of conditions is that which we comprehend under the general name *generosity*. The position of M. de Laveleye, and of the multitudes who think with him, is that the community, through its government, may rightly undertake both to administer justice and to practise generosity. On the other hand, I, and the few who think with me, contend that justice alone is to be administered by the community in its corporate capacity; and that the practice of generosity is to be left to private individuals, and voluntarily-formed combinations of individuals. Insuring each citizen's safety in person and property, as well as insuring him

such returns for his services as his fellow-citizens agree to give, is a public affair; while affording him help, and giving him benefits beyond those he has earned, is a private affair. The reason for maintaining this distinction is that the last duty cannot be undertaken by the State without breach of the first. The vital requirement to social life must be broken that a non-vital requirement may be fulfilled. Under a reign of absolute justice unqualified by generosity, a social life may be carried on, though not the highest social life; but a reign of generosity without any justice—a system under which those who work are not paid, so that those who have been idle or drunken may be saved from misery—is fatal; and any approach to it is injurious. That only can be a wholesome state in which conduct brings its natural results, good or evil, as the case may be; and it is the business of Government, acting on behalf of all, to see that each citizen shall not be defrauded of the good results, and that he shall not shoulder off the evil results on to others. If others, in their private capacities, are prompted by affection or pity to mitigate the evil results, by all means let them do so: no power can equitably prevent them from making efforts, or giving money, to diminish the sufferings of the unfortunate and the inferior; at the same time that no power can equitably coerce them into doing this.

If M. de Laveleye holds, as he appears to do, that enforcing the normal relations between conduct and consequences, right as it may be in the abstract, is impracticable under existing social conditions, which are in many cases such that men get what they have neither earned nor otherwise equitably received, and in many cases such that they are prevented from earning anything; then my reply is, by all means, where this condition of things is due to unjust arrangements, let us rectify these arrangements as fast as we can. But let us not

adopt the disastrous policy of establishing new injustices for the purpose of mitigating the mischiefs produced by old injustices.

GOVERNMENT BY MINORITY.

The Irish party in 1885, under the leadership of Mr. Parnell, carried on an organized system of obstruction, the aim of which was to stop all legislation until Home Rule had been granted. The immediate question was that which exclusively occupied attention, but it seemed desirable to draw attention to a remoter question which was involved; and to this end I published the following letter in *The Times* for December 21, 1885.

Amid minor political issues occupying all minds the major political issue passes unnoticed.

The major political issue is—shall we maintain the supremacy of majorities? While in theory asserting it more emphatically than ever, we are in practice meanly relinquishing it. The very moment after we have extended the system of government by majority outside the House, we are tacitly allowing the system of government by minority inside the House. We are helplessly looking forward to the coercion of two great parties by one small party.

Right feeling alone, or moderate intelligence alone, should have sufficed to make such a thing impossible, much more the two united. This impudent dictation by the few to the many might have been expected to rouse in the many a just anger, great enough to make them sink all party differences while jointly resisting

it; and it might have been expected that Liberals and Conservatives alike, without any high stretch of intellect, would have seen that, deeper than any legislative question which divides them, is the question whether they shall allow the principle on which all our legislation is founded to be contemptuously broken through.

Thirty years ago Prince Albert gave great offence by saying that representative government was on its trial. We are now approaching a supreme moment when its trial threatens to end in lamentable failure. If this failure occurs—if the 584 allow themselves to be coerced by the 86—then the 584 will be traitors to free institutions.

EVOLUTIONARY ETHICS.

The following letter, published in the *Athenæum* for August 5, 1893, was drawn from me in response to certain passages in the Romanes Lecture, delivered by the late Prof. Huxley at Oxford in the Spring of 1893. These passages were supposed to be directed against doctrines I hold (see *Athenæum*, July 22, 1893); and it seemed needful that I should defend myself against an attack coming from one whose authority was so great. My justification for including this letter among these fragments is that since the Romanes Lecture referred to exists in a permanent form, it is proper that a permanent form should be given to my reply.

If it is not too great a breach of your rules, will you allow me space for some remarks suggested by the review of Prof. Huxley's lecture on "Evolution and Ethics," con-

tained in your issue of the 22nd inst.?

The incongruity between note 19 of the series appended to the lecture, and a leading doctrine contained in the lecture itself, is rightly pointed out by your reviewer. In the lecture Prof. Huxley says:—

“The practice of that which is ethically best—what we call goodness or virtue—involves a course of conduct which, in all respects, is opposed to that which leads to success in the cosmic struggle for existence. In place of ruthless self-assertion it demands self-restraint.”—P. 33.

But in note 19 he admits that—

“strictly speaking [why not rightly speaking?], social life and the ethical process, in virtue of which it advances towards perfection, are part and parcel of the general process of evolution, just as the gregarious habit of innumerable plants and animals, which has been of immense advantage to them, is so.”

I do not see how the original assertion can survive after this admission has been made. Practically the last cancels the first. If the ethical process is a part of the process of evolution or cosmic process, then how can the two be put in opposition? Prof. Huxley says:—

“The struggle for existence, which has done such admirable work in cosmic nature, must, it appears [according to the view he opposes], be equally beneficent in the ethical sphere. Yet, if that which I have insisted upon is true; if the cosmic process has no sort of relation to moral ends; if the imitation of it by man is inconsistent with the first principles of ethics; what becomes of this surprising theory?” P. 34.

But when we find that the hypothetical statement, “if the cosmic process has no sort of relation to moral ends,” is followed by the positive statement that “the cosmic process” *has* “a sort of relation to moral ends,” we may ask, “what becomes of this surprising” criticism? Obviously, indeed, Prof. Huxley cannot avoid admitting that the ethical process, and, by implication, the ethical

man, are products of the cosmic process. For if the ethical man is not a product of the cosmic process, what is he a product of?

The view of which Prof. Huxley admits the truth in note 19 is the view which I have perpetually enunciated: the difference being that instead of relegating it to an obscure note, I have made it a conspicuous component of the text. As far back as 1850, when I did not yet recognize evolution as a process co-extensive with the cosmos, but only as a process exhibited in man and in society, I contended that social progress is a result of "the ethical process," saying that—

"the ultimate man will be one whose private requirements coincide with public ones. He will be that manner of man who, in spontaneously fulfilling his own nature incidentally performs the functions of a social unit; and yet is only enabled so to fulfil his own nature, by all others doing the like."—*Social Statics*, "General Considerations."

And from that time onwards I have, in various ways, insisted upon this truth. In a chapter of the *Principles of Ethics* entitled "Altruism versus Egoism," it is contended that from the dawn of life altruism of a kind (parental altruism) has been as essential as egoism; and that in the associated state the function of altruism becomes wider, and the importance of it greater, in proportion as the civilization becomes higher. Moreover, I have said that—

"from the laws of life it must be concluded that unceasing social discipline will so mould human nature, that eventually sympathetic pleasures will be spontaneously pursued to the fullest extent advantageous to each and all." *Ethics*, § 95.

"With the highest type of human life, there will come also a state in which egoism and altruism are so conciliated that the one merges in the other."—*Ib.*, appended chapter to Part I.

Everywhere it is asserted that the process of adaptation (which, in its direct and indirect forms, is a part of the cosmic process) must continuously tend (under peaceful conditions) to produce a type of society and a type of individual in which "the instincts of savagery in civilized men" will be not only "curbed," but repressed. And I believe that in few, if any, writings will be found as unceasing a denunciation of that brute form of the struggle for existence which has been going on between societies, and which, though in early times a cause of progress, is now becoming a cause of retrogression. No one has so often insisted that "the ethical process" is hindered by the cowardly conquests of bullet and shell over arrow and assegai, which demoralize the one side while slaughtering the other.

And here, while referring to the rebarbarizing effects of the struggle for existence carried on by brute force, let me say that I am glad to have Prof. Huxley's endorsement of the proposition that the survival of the fittest is not always the survival of the best. Twenty years ago, in an essay entitled "Mr. Martineau on Evolution," I pointed out that "the fittest" throughout a wide range of cases—perhaps the widest range—are not the "best"; and said that I had chosen the expression, "survival of the fittest" rather than survival of the best because the latter phrase did not cover the facts.

Chiefly, however, I wish to point out the radical misconceptions which are current concerning that form of evolutionary ethics with which I am indented. In the preface to *The Data of Ethics*, when first published separately, I remarked that by treating the whole subject in parts, which would by many be read as though they were wholes, I had "given abundant opportunity for misrepresentation." The opportunity has not been lost. The division treating of "Justice" has been habitually spoken of as though nothing more was intended to be said; and

this notwithstanding warnings which the division itself contains, as in § 257, and again in § 270; where it is said that "other injunctions which ethics has to utter do not here concern us..... there are the demands and restraints included under Negative Beneficence and Positive Beneficence, to be hereafter treated of." Even if considered apart, however, the doctrine set forth in this division has no such interpretation as that perversely put upon it. It is represented as nothing but an assertion of the claims of the individual to what benefits he can gain in the struggle for existence; whereas it is in far larger measure a specification of the equitable limits to his activities, and of the restraints which must be imposed on him. I am not aware that any one has more emphatically asserted that society in its corporate capacity must exercise a rigorous control over its individual members, to the extent needful for preventing trespasses one upon another. No one has more frequently or strongly denounced governments for the laxity with which they fulfil this duty. So far from being, as some have alleged, an advocacy of the claims of the strong against the weak, it is much more an insistence that the weak shall be guarded against the strong, so that they may suffer no greater evils than their relative weakness itself involves. And no one has more vehemently condemned that "miserable *laissez-faire* which calmly looks on while men ruin themselves in trying to enforce by law their equitable claims" (*Ethics*, § 271).

Now that the remaining parts, treating of Beneficence, have been added to the rest, the perverse misinterpretation continues in face of direct disproofs. At the very outset of the *Ethics* is said:—

"There remains a further advance not yet even hinted. For beyond so behaving that each achieves his ends without preventing others from achieving their ends, the members of a society may give mutual help in the achieve-

ment of ends." § 6.

And in a subsequent chapter it is said that

"the limit of evolution of conduct is consequently not reached until, beyond avoidance of direct and indirect injuries to others, there are spontaneous efforts to further the welfare of others." "It may be shown that the form of nature which thus to justice adds beneficence, is one which adaptation to the social state produces."—§ 54.

These are texts which in Parts V. and VI., dealing with Beneficence, Negative and Positive, are fully expanded. Having first distinguished between "kinds of altruism," and contended that the kind we call justice has to be enforced by the incorporated society, the State, while the kind we call beneficence must be left to individuals, and after pointing out the grave evils which result if this distinction is not maintained, I have described in detail the limits to men's actions which negative beneficence enjoins. Then come two chapters, entitled "Restraints on Free Competition" and "Restraints on Free Contract," respectively indicating various cases in which the restraints imposed by law must be supplemented by self-restraints, and instancing one of the excesses committed under free competition as amounting to "commercial murder." Chapters enjoining further self-restraints for the benefit of others are followed, in the division on Positive Beneficence, by chapters enjoining efforts on their behalf, and the duty which falls on the superior of mitigating the evils which the inferior have to bear. After dealing, in a chapter on "Relief of the Poor," with the evils often caused by attempts to diminish distress, it is contended that philanthropic duty should be performed not by proxy, but directly; and that each person of means ought to see to the welfare of the particular cluster of inferiors with whom his circumstances put him in relation. The general nature of the doctrine set forth may be inferred from two sentences in

the closing chapter:—

“The highest beneficence is that which is not only prepared, if need be, to sacrifice egoistic pleasures, but is also prepared, if need be, to sacrifice altruistic pleasures.”—§ 474.

And then, speaking of the natures which “the ethical process” is in course of producing, it is said that

“in such natures a large part of the mental life must result from participation in the mental lives of others.”—§ 475.

I do not see how there could be expressed ideas more diametrically opposed to that brutal individualism which some persons ascribe to me.

It remains only to say that Prof. Huxley’s attack upon the doctrines of Ravachol & Co. has my hearty approval, though I do not quite see the need for it. Evidently it is intended for the extreme anarchists; or, at least, I know of no others against whom his arguments tell. It has been absurdly supposed that his lecture was, in part, an indirect criticism upon theories held by me. But this cannot be. It is scarcely supposable that he deliberately undertook to teach me my own doctrines, enunciated some of them forty-odd years ago. Passing over the historical and metaphysical parts of his lecture, his theses are those for which I have always contended. We agree that the process of evolution must reach a limit, after which a reverse change must begin (*First Principles*, chaps. “Equilibration” and “Dissolution”). We agree that the survival of the fittest is often not survival of the best. We agree in denouncing the brutal form of the struggle for existence. We agree that the ethical process is a part of the process of evolution. We agree that the struggle for life needs to be qualified when the gregarious state is entered, and that among gregarious creatures lower than man a rudiment of the ethical check is visible. We agree that among men the ethical check, becoming

more and more peremptory, has to be enforced by the society in its corporate capacity, the State. We agree that beyond that qualification of the struggle for life which consists in restricting the activities of each so that he may not trench upon the spheres for the like activities of others, which we call justice, there needs that further qualification which we call beneficence; and we differ only respecting the agency by which the beneficence should be exercised. We agree in emphasizing, as a duty, the effort to mitigate the evils which the struggle for existence in the social state entails; and how complete is this agreement may be seen on observing that the sentiment contained in Prof. Huxley's closing lines is identical with the sentiment contained in the last paragraph of the *Principles of Ethics*. Obviously, then, it is impossible that Prof. Huxley can have meant to place the ethical views he holds in opposition to the ethical views I hold; and it is the more obviously impossible because, for a fortnight before his lecture, Prof. Huxley had in his hands the volumes containing the above quotations, along with multitudinous passages of kindred meanings. But as this erroneous belief is prevalent, it seems needful for me to dissipate it. Hence this letter.

The closing lines of this last paragraph were regarded by Prof. Huxley as tacitly charging him with an unacknowledged adoption of my views. It did not occur to me when writing them that they could be so interpreted. My intention was simply to show that he had abundant opportunity for seeing at first hand what my views were, and had therefore the less reason for presenting his own similar views as though they stood in opposition to mine.

SOCIAL EVOLUTION AND SOCIAL DUTY.

It is quite by accident that this fragment succeeds the last in order of date, and when it was written the last was not in my thoughts. The sequence, however, is fortunate. Its cardinal idea is similar to that contained in my reply to Prof. Cairnes already given; but as it is differently presented, and as it is one which many find it difficult to grasp, it seems desirable to repeat it in this recast form. A Congress of Evolutionists was held at Chicago on Sept. 28, 29 and 30, 1893; and this brief paper was sent in response to a request to contribute to its proceedings.

At a congress which has for its chief purpose to advance ethics and politics by diffusing evolutionary ideas it seems especially needful to dissipate a current misconception respecting the relation in which we stand individually towards the process of social evolution. Errors of a certain class may be grouped as errors of the uncultured, but there are errors of another class which characterize the cultured—implying, as they do, a large amount of knowledge with a good deal of thought but yet with thought not commensurate with the knowledge. The errors I refer to are of this class.

The conception of evolution at large, as it exists in those who are aware that evolution includes much more than "natural selection," involves the belief that from beginning to end it goes on irresistibly and unconsciously. The concentration of nebulae into stars and the formation of solar systems are determined entirely by certain properties of the matter previously diffused. Planets which were once gaseous, then liquid, and finally covered

by their crusts, gradually undergo geological transformations in virtue of mechanical and chemical processes.

Similarly, too, when we pass to organic bodies, plant and animal. Enabled to develop individually, as they are, by environing forces, and enabled to develop as species by processes which continue to adapt and readapt them to their changing environments, they are made to fit themselves to their respective lives and, along certain lines, to reach higher lives, purely by the involved play of forces of which they are unconscious. The conception of evolution at large, thus far correct, is by some extended to that highest form of evolution exhibited in societies. It is supposed that societies, too, passively evolve apart from any conscious agency; and the inference is that, according to the evolutionary doctrine, it is needless for individuals to have any care about progress, since progress will take care of itself. Hence the assertion that "evolution erected into the paramount law of man's moral and social life becomes a paralyzing and immoral fatalism."

Here comes the error. Everyone may see that throughout the lower forms of evolution the process goes on only because the various units concerned—molecules of matter in some cases, and members of a species in another—respectively manifest their natures. It would be absurd to expect that inorganic evolution would continue if molecules ceased to attract or combine, and it would be absurd to suppose that organic evolution would continue if the instincts and appetites of individuals of each species were wholly or even partially suspended.

No less absurd is it to expect that social evolution will go on apart from the normal activities, bodily and mental, of the component individuals—apart from their desires and sentiments, and those actions which they prompt. It is true that much social evolution is achieved without any intention on the part of citizens to achieve it, and

even without the consciousness that they are achieving it. The entire industrial organization in all its marvellous complexity, has arisen from the pursuit by each person of his own interests, subject to certain restraints imposed by the incorporated society; and by this same spontaneous action have arisen also the multitudinous appliances of industry, science and art, from flint knives up to automatic printing machines, from sledges up to locomotives—a fact which might teach politicians that there are at work far more potent social agencies than those which they control.

But now observe that just as these astonishing results of social evolution, under one of its aspects, could never have arisen if men's egoistic activities had been absent, so in the absence of their altruistic activities there could never have arisen and cannot further arise certain higher results of social evolution. Just as the egoistic feelings are the needful factors in the one case, so the altruistic feelings are the needful factors in the other, and whoever supposes the theory of evolution to imply that advanced forms of social life will be reached even if the sympathetic promptings of individuals cease to operate, does not understand what the theory is.

A simple analogy will make the matter clear. All admit that we have certain desires which insure the maintenance of the race—that the instincts which prompt to the marital relation and afterwards subserve the parental relation make it certain that, without any injunction or compulsion, each generation will produce the next. Now suppose some one argued that since, in the order of nature, continuance of the species was thus provided for, no one need do anything towards furthering the process by marrying. What should we think of his logic—what should we think of his expectation that the effect would be produced when the causes of it were suspended?

Yet absurd as he would be, he could not be more absurd

than the one who supposed that the higher phases of social evolution would come without the activity of those sympathetic feelings in men which are the factors of them—or rather, he would not be more absurd than one who supposed that this is implied by the doctrine of evolution.

The error results from failing to see that the citizen has to regard himself at once subjectively and objectively—subjectively as possessing sympathetic sentiments (which are themselves the products of evolution); objectively as one among many social units having like sentiments, by the combined operation of which certain social effects are produced. He has to look on himself individually as a being moved by emotions which prompt philanthropic actions, while, as a member of society, he has to look on himself as an agent through whom these emotions work out improvements in social life. So far, then, is the theory of evolution from implying a “paralyzing and immoral fatalism,” it implies that, for genesis of the highest social type and production of the greatest general happiness, altruistic activities are essential as well as egoistic activities, and that a due share in them is obligatory upon each citizen.

PARLIAMENTARY GEORGITES.

While the Parish Councils Bill was before Parliament, I made the following comment, under the title of “Parliamentary Georgites” on the general character of its provisions, which was published in *The Times* for February 20, 1894. I had long wished to express the opinion that the great majority of Englishmen, abandoning

as they have done the teachings of political economists and those who a generation ago had diffused rational ideas concerning the State and its functions, are politically drunk; and I here seized the occasion for indicating this opinion by signing this letter—"One who is still sober."

How Mr. Henry George must chuckle as he reads about the doings of the English House of Commons! To think that already he should have obtained the majority of that assemblage as converts to his leading doctrine!

To his leading doctrine? Well, if not to the doctrine, yet to the method by which he proposes to carry out the doctrine. "We must not turn the landlords out, we must tax them out," has been his injunction for years past, and our legislators are obeying his injunction.

From the time when the compound householder came into existence it has become manifest *à posteriori*, as it was manifest *à priori*, that giving public power to men without imposing on them public burdens leads to extravagance and injustice. Under municipal governments, practically elected by non-ratepayers (for these can turn the scale), lavish expenditure, rising rates, and the piling up of vast debts have proceeded with increasing rapidity. Under rural governments, similarly elected, are we not to expect similar results?

"Trust the people," they say. Certainly, trust the people to do that which human beings in general do—follow their own ends. When "the classes" were predominant it was rightly complained that they dealt unjustly with "the masses." Now that "the masses" are predominant, will they not deal unjustly with "the classes"? If the first were biased by their interests, will not the second also be biased by their interests? And what will be their interests? To get as many benefits as possible

given to them out of public funds to which they do not contribute—do not consciously contribute.

Doubtless Mr. Henry George knows that before the Revolution one-fourth of France was made valueless by the weight of taxation and became waste. Probably he knows, too, that under our old Poor Law the rates had in some parishes risen to half the rental, and that in one Buckinghamshire parish they had absorbed the whole proceeds of the soil—owners' rents, occupiers' profits—and that the rector, having given up his glebe and tithes, proposed that all the land should be divided among the paupers.

Perhaps Mr. George will infer that, if this could happen when the rate-eaters had no power of levying rates, far more readily will it happen when those who get gratis benefits from rates will have part power and often the chief power of levying rates. And if he infers this, we may imagine the sardonic grin with which he watches some hundreds of propertied representatives complacently smoothing the way for the Socialists.

There are moral epidemics as well as physical epidemics; and the moral influence, or influenza, which now prevails so widely has a symptom in common with its physical analogue—it is accompanied by nervous prostration. Those seized by it are smitten with paralysis of reason. For how else can we account for the astounding fact that, day by day, the select men of the nation are empowering those who own nothing to say to those who own something, "We will decide what shall be done, and you shall pay for it." Actually it has come to this—that "collective wisdom" thinks society will prosper under that principle!

A RECORD OF LEGISLATION.

The project described in the following letter, which appeared in *The Times* for Nov. 24, 1894, is one I had long entertained; and the incident referred to in its opening paragraph prompted me no longer to delay setting it forth.

The attention which has been drawn to Mr. Ilbert's proposal for a record of comparative legislation suggests to me the propriety of naming a project akin to it towards the execution of which a small step has been made.

The project I refer to was originally conceived as a kind of supplement to the "Descriptive Sociology" (or rather to one division of it), and might eventually have been entered upon had not the heavy losses year by year entailed on me by that compilation obliged me to discontinue it. The end in view was to present briefly, in a tabulated form, the contents of our Statute-book from early days onwards, showing why each law was enacted, the effects produced, the duration, and, if repealed, the reasons for the repeal; the general purpose being that of making easily accessible the past experience useful for present guidance. The scheme in its developed form included like tabulations of the laws of other nations, which, while making comparisons possible, would enable us to profit by other legislative experiments than those of our ancestors. There was, however, no thought of dealing in like manner with the legislation of the English-speaking races at large.

In 1887 a tentative step was taken towards execution of this scheme. There existed at that time a weekly publication entitled *Jus*, established and edited by Mr. Wordsworth Donisthorpe, and partly devoted to the exposure of mischievous law-making. In pursuance of a

suggestion which he says I made to him in 1873, he commenced giving instalments of such a digest as that described above; and these instalments were continued from September, 1887, to March, 1888, when the death of the periodical brought them to a close.

A further step was subsequently taken. Between two and three years ago I named the project to a philanthropic millionaire, and the interest he displayed in it led me to think that he would furnish funds for carrying it out. That he might be able to decide, however, it was needful that a finished portion of such a digest should be produced, and, in consultation with Mr. Donisthorpe, a final form of table was agreed upon. Prompted by the expectation raised, Mr. Donisthorpe enlisted in the cause Mr. J. C. Spence, by whose labours, aided by his own, a table was duly prepared, put in type, and printed. As is shown by the enclosed copy of this printed table, its parallel columns, severally filled up, are headed:—"Reasons for the Enactment"; "Provisions of Enactment"; "Date and Title"; "Effects"; "Repeal." The period dealt with extended from 1328 to 1349; and the table showed that nearly all the laws passed have been repealed.

To complete the conception of the scheme it should be added that along with the whole series of tables, thus sampled, there was to be a subject-index, so classified into divisions and sub-divisions of matters dealt with by law, that by reference to any particular division or sub-division, and then to the pages named as containing the laws relating to it, it would be possible in a few minutes to learn what has been attempted in successive centuries in respect of any particular matter and with what results.

Unfortunately, however, when this sample table was put before my millionaire friend he expressed the opinion that he could devote his surplus revenues to purposes of more importance. The project thus dropped and

nothing further has since been done.

I am fully conscious that no such compilation could be made complete. Doubtless numerous Acts dealing with trivial matters would have to be omitted to prevent undue voluminousness; and it is certain that in many cases the effects produced by Acts could not be definitely stated; though in these cases the mere fact of repeal would often have sufficient significance. But it is not, therefore, to be concluded that an undertaking of this kind, imperfectly executed though it might be, should not be carried out as far as possible. It is true that politicians and legislators who plume themselves on being "practical," and whose facts are furnished by Blue-books and Parliamentary Debates, would probably pay but small respect to these groups of facts furnished by the legislative experiments of our forefathers. Experiences of the day satisfy them. But those who take wider views and see that generalizations drawn from the entire past life of a nation are more to be trusted than these superficial generalizations, and that it is folly to make laws without inquiring what have been the results of essentially similar laws long ago passed and long ago abandoned, will see that such a work, containing easily accessible information, might have considerable effect in preventing some of the legislative blunders which are daily made.

It is more for the purpose of putting this project on record than with the hope that it may be executed in our day that I write this letter. The ambitions which now prevail among the wealthy, and in fulfilment of which they spend large sums, may hereafter be replaced by ambitions of a higher kind, and then the needful funds may be forthcoming.

In a leading article commenting on this letter, which appeared simultaneously, it was objected that it would be impracticable to ascertain the good or evil effects of

past laws and the reasons why they had been repealed. Doubtless in many cases ascertainment would be difficult and even impossible. But if, as a general rule, the effects of laws cannot be ascertained, then it becomes impossible to distinguish between good and bad laws; and if laws cannot be classed as good or bad by their ascertained effects, then one law is as good as another and legislation becomes meaningless. Even without pressing this logical implication it may be replied that we ought to know what things have been attempted by laws and in what cases repeal soon followed or enforcement was found impracticable.

In the hope that hereafter some man or men of adequate means will see that such a record, partially if not wholly practicable, would be of high value, I here append the sample table above referred to as having been arranged and filled up.

ANGLO-AMERICAN ARBITRATION.

On March 3, 1896, a short time before official steps were taken towards the establishment of permanent International Arbitration with the United States, a demonstration in furtherance of that end was held at Queen's Hall, Sir James Stansfeld in the chair. In response to an appeal I sent the following letter, which was read at the meeting.

Were it not that ill-health obliges me to shun all excitements, I should gladly attend the meeting to be held this evening at Queen's-hall in support of Anglo-American arbitration. As it is, I can do no more than emphatically express approval of its aims.

Savage as have been the passions commonly causing war, and great as have been its horrors, it has, throughout the past, achieved certain immense benefits. From it has resulted the predominance and spread of the most powerful races. Beginning with primitive tribes it has welded together small groups into larger groups, and again at later stages has welded these larger groups into still larger, until nations have been formed. At the same time military discipline has habituated wild men to the bearing of restraints, and has initiated that system of graduated subordination under which all social life is carried on. But though, along with detestation of the cruelties and bloodshed and brutalization accompanying war, we must recognize these great incidental benefits bequeathed by it heretofore, we are shown that henceforth there can arise no such ultimate good to be set against its enormous evils. Powerful types of men now possess the world; great aggregates of them have been consolidated; societies have been organized; and throughout the future the conflicts of nations, entailing on larger scales than ever before death, devastation, and misery, can yield to posterity no compensating advantages. Henceforth social progress is to be achieved, not by systems of education, not by the preaching of this or that religion, not by insistence on a humane creed daily repeated and daily disregarded, but only by cessation from these antagonisms which keep alive the brutal elements of human nature, and by persistence in a peaceful life which gives unchecked play to the sympathies. In sundry places, and in various ways, I have sought to show that advance to higher forms of man and society

essentially depends on the decline of militancy and the growth of industrialism. This I hold to be a political truth in comparison with which all other political truths are insignificant.

I need scarcely add that such being my belief, I rejoice over the taking of any step which directly diminishes the probability of war, and indirectly opens the way to further such steps.

AGAINST THE METRIC SYSTEM.

During the Parliamentary Session of 1896 an association which has for some time past sought to establish the Metric System in England, had obtained from the Parliamentary Secretary of the Board of Trade, a promise that a Bill conforming to their desire should be presently introduced. Holding strongly the opinion that adoption of the Metric System is undesirable, I published in *The Times*, as special articles "From a Correspondent," four letters setting forth the reasons for this opinion; and immediately afterwards issued these letters in the form of a pamphlet, which was distributed to all members of the House of Commons and a few members of the House of Lords here, and also to members of the United States Congress, before which a Bill to establish the Metric System in America was pending. The contents of this pamphlet, including certain explanatory lines introducing the letters, are now reproduced.

On the 20th inst., in answer to a question, Mr. Balfour

implied that the Government did not contemplate compulsory enactment of the metric system. At that date this pamphlet was in the press, and I was at first inclined to stay further progress; thinking that issue of it would be superfluous. Second thoughts, however, led to persistence.

On the 24th March, at the Annual Meeting of the Associated Chambers of Commerce, a motion urging adoption of the metric system was carried; and the Earl of Dudley, Parliamentary Secretary of the Board of Trade, responding to its embodied wish, announced that "a Bill was now in course of preparation which would be brought in at no distant date, and which would give effect to the wishes expressed in the motion." The provisions of such a Bill, should it be brought forward, will be subject to criticisms irrespective of their characters as compulsory or permissive. Hence it seems still desirable to bring together, in a convenient form for reference, the facts and arguments which go to show that the metric system is ill-adapted for industrial and trading purposes.

Of the four following letters, the first, which discusses the claims of the English yard *versus* the French mètre, may be passed over by those who have little time for reading, since it does not essentially concern the main issue.

I.—Advocates of the metric system allege that all opposition to it results from "ignorant prejudice." This is far from being the fact. There are strong grounds for rational opposition, special and general; some already assigned and others which remain to be assigned. I may fitly put first a carefully-reasoned expression of dissent from a late man of science of high authority.

In 1863 Sir John Herschel published an essay in which, after referring to an attempt made during the preceding Session to carry through Parliament a Bill establishing the French metric system in this country, and anticipating

that the Bill (said to have been confirmed in principle) would be again brought forward, he proceeded to contrast that system with a better one to be reached by making a minute modification in our own unit of measure. The following extract will sufficiently indicate the line of his argument:—

“Let us now see how far the French *mètre* as it stands fulfils the requirements of scientific and ideal perfection. It professes to be the 10,000,000th part of the quadrant of the meridian passing through France from Dunkirk to Formentera, and is, therefore, scientifically speaking, a local and national and not a universal measure..... The *mètre*, as represented by the material standard adopted as its representative, is too short by a sensible and measurable quantity, though one which certainly might be easily corrected.”

[In the appendix it is shown that according to the latest measurements the error is 1-163rd part of an inch on the *mètre*.]

Sir John goes on to say that “were the question an open one what standard a new nation, unprovided with one and unfettered by usages of any sort, should select, there could be no hesitation as to its adoption (with that very slight correction above pointed out)”; and he then continues—

“The question now arising is quite another thing, viz. :— Whether we are to throw overboard an existing, established, and, so to speak, ingrained system—adopt the *mètre* as it stands for our standard—adopt, moreover, its decimal subdivisions, and carry out the change into all its train of consequences, to the rejection of our entire system of weights, measures, and coins. If we adopt the *mètre* we cannot stop short of this. It would be a standing reproach and anomaly—a change for changing’s sake. The change, if we make it, must be complete and thorough. And this is in the face of the fact that England is beyond all question the nation whose commercial relations, both internal and external, are the greatest in the world, and that the British system of measures is re-

ceived and used, not only throughout the whole British Empire (for the Indian 'Hath' or revenue standard is defined by law to be 18 British Imperial inches), but throughout the whole North American continent, and (so far as the measure of length is concerned) also throughout the Russian Empire..... Taking commerce, population, and area of soil then into account, there would seem to be far better reason for our Continental neighbours to conform to our linear unit could it advance the same or a better *a priori* claim, than for the move to come from our side. (I say nothing at present of decimalization.)"

Sir John Herschel then argues that the 10,000,000th part of the quadrant of a meridian, which is the specified length of the mètre, is, on the face of it, not a good unit of measure, inasmuch as it refers to a natural dimension not of the simplest kind, and he continues thus:—

"Taking the polar axis of the earth as the best unit of dimension which the terrestrial spheroid affords (a better *a priori* unit than that of the metrical system), we have seen that it consists of 41,708,088 imperial feet, which, reduced to inches, is 500,497,056 imperial inches. Now this differs only by 2,944 inches, or by 82 yards, from 500,500,000 such inches, and this would be the whole error on a length of 8,000 miles, which would arise from the adoption of this precise round number of inches for its length, or from making the inch, so defined, our fundamental unit of length.

After pointing out that the calculation required for correlating a dimension so stated with the Earth's axis, is shorter than that required for correlating a kindred dimension with the quadrant of a meridian, Sir John Herschel argues that—

"If we are to legislate at all on the subject, then the enactment ought to be to increase our present standard yard (and, of course, all its multiples and submultiples) by one precise thousandth part of their present lengths, and we should then be in possession of a system of linear

measure the purest and the most ideally perfect imaginable. The change, so far as relates to any practical transaction, commercial, engineering, or architectural, would be absolutely unfelt, as there is no contract for work even on the largest scale, and no question of ordinary mercantile profit or loss, in which one *per mille* in measure or in coin would create the smallest difficulty."

"Hitherto I have said nothing about our weights and measures of capacity. Now, as they stand at present, nothing can be more clumsy and awkward than the numerical connection between these and our unit of length."

And then, after pointing out the way in which the slight modification of the unit of linear measure described by him, could be readily brought into such relation with the measures of capacity and weight as to regularize them, he goes on:—

"And thus the change which would place our system of linear measure on a perfectly faultless basis would, at the same time, rescue our weights and measures of capacity from their present utter confusion."

In presence of the opinion thus expressed, and thus supported by evidence, we ought, I think, to hear nothing more about "ignorant prejudice" as the only ground for opposition to the metric system, now being urged upon us. But, before proceeding to give adverse reasons of my own, let me quote a further objection—not, it may be, of the gravest kind, but one which must be taken into account. Writing from Washington, Professor H. A. Hazen, of the United States Weather Bureau, published in *Nature* of January 2, this year, a letter of which the following extracts convey the essential points:—

"The metric system usually carries with it the Centigrade scale on the thermometer, and here the whole English-speaking world should give no uncertain sound. In meteorology it would be difficult to find a worse scale than the Centigrade. The plea that we must have just 100° between the freezing and boiling points does not

hold; any convenient number of degrees would do. The Centigrade degree ($1^{\circ}3f.$) is just twice too large for ordinary studies. The worst difficulty, however, is in the use of the Centigrade scale below freezing. Any one who has had to study figures half of which have *minus* signs before them knows the amount of labour involved. To average a column of 30 figures half of which are *minus* takes nearly double the time that figures all on one side would take, and the liability to error is more than twice as great. I have found scores of errors in foreign publications where the Centigrade scale was employed, all due to this most inconvenient *minus* sign. If any one ever gets a 'bee in his bonnet' on this subject and desires to make the change on general principles it is very much to be hoped that he will write down a column of 30 figures half below 32° F., then convert them to the Centigrade scale, and try to average them. I am sure no English meteorologist who has ever used the Centigrade scale will ever desire to touch it."

But, now having noted these defects, which may perhaps be considered defects of detail, since they do not touch the fundamental principle of the metric system, I propose, with your permission, to show that its fundamental principle is essentially imperfect and that its faults are great and incurable.

II.—In reply to my inquiries, a French friend, member of the Conseil d'État, after giving instances of nonconformity to the metric system, ended by saying:—"En adoptant le système métrique décimal, on n'a pas fait disparaître tout à fait les dénominations anciennes, mais on en a fortement réduit l'emploi."

It is now more than a century since, in the midst of the French revolution, the metric system was established. Adoption of it has been in the main compulsory. As French citizens have been obliged to use francs and centimes, so must they have been obliged to use the State-authorized weights and measures. But the implication

of the above statement is that the old customs have survived where survival was possible: the people can still talk in sous and ask for fourths, and they do so. Doubtless "ignorant prejudice" will be assigned as the cause for this. But one might have thought that, after three generations, daily use of the new system would have entailed entire disappearance of the old, had it been in all respects better.

Allied evidence exists. While in the land of its origin the triumph of the metric system is still incomplete, in one of the lands of its partial adoption, the United States, the system has been departed from. It will be admitted that men engaged in active business are, by their experience, rendered the best judges of convenience in monetary transactions; and it will be admitted that a Stock Exchange is, above all places, the focus of business where facilitation is most important. Well, what has happened on the New York Stock Exchange? Are the quotations of prices in dollars, tenths, and cents? Not at all. They are in dollars, halves, quarters, eighths; and the list of prices in American securities in England shows that on the English Stock Exchange quotations are not only in quarters and eighths, but in sixteenths and even thirty-seconds. That is to say, the decimal divisions of the dollar are in both countries absolutely ignored, and the division into parts produced by halving, re-halving, and again halving is adopted. Worse has happened. A friend writes:—"When I was in California some 20 years ago the ordinary usage was to give prices in 'bits,' the eighth of a dollar—a 'long bit' was 15 cents, a 'short bit' was 10 cents. If one had a long bit and paid it one got no change—if one gave a short one no supplement was asked." Thus, lack of appropriate divisibility led to inexact payments—a retrogression.

Perhaps an imaginary dialogue will most conveniently bring out the various reasons for dissent. Let us suppose

that one who is urging adoption of the metric system, is put under cross-examination by a sceptical official. Some of his questions might run thus:—

What do you propose to do with the circle? At present it is divided into 360 degrees, each degree into 60 minutes, and each minute into 60 seconds. I suppose you would divide it into 100 degrees, each degree into 100 minutes, and each of these into 100 seconds?

The French have decimalized the quadrant, but I fear their division will not be adopted. Astronomical observations throughout a long past have been registered by the existing mode of measurement, and works for nautical guidance are based upon it. It would be impracticable to alter this arrangement.

You are right. The arrangement was practically dictated by Nature. The division of the circle was the outcome of the Chaldean division of the heavens to fit their calendar: a degree being, within 1-60th, equivalent to a day's apparent motion of the Sun on the ecliptic. And that reminds me that I do not find in your scheme any proposal for re-division of the year. Why do you not make 10 months instead of 12?

A partial decimalization of the calendar was attempted at the time of the French Revolution: a week of ten days was appointed, but the plan failed. Of course, the 365 days of the year do not admit of division into tenths; or if ten months were made, there could be no tenths of these. Moreover, even were it otherwise, certain deeply-rooted customs stand in the way. Many trading transactions, especially the letting of houses and the hiring of assistants, have brought the quarter-year into such constant use that it would be very difficult to introduce a re-division of the year into tenths.

Just so; and it occurs to me that there is a deeper reason. Ignoring the slight ellipticity of the Earth's orbit, a quarter of a year is the period in which the Earth

describes a fourth of its annual journey round the Sun, and the seasons are thus determined—the interval between the shortest day and the vernal equinox, between that and the longest day, and so on with the other divisions.

The order of Nature is doubtless against us here.

It is against you here in a double way. Not only the behaviour of the Earth, but also the behaviour of the Moon conflicts with your scheme. By an astronomical accident it happens that there are 12 full moons, or approximately 12 synodic lunations, in the year; and this, first recognized by the Chaldeans, originated the 12-month calendar, which civilized peoples in general have adopted after compromising the disagreements in one or other way. But there is another division of time in which you are not so obviously thus restrained. You have not, so far as I see, proposed to substitute 10 hours for 12, or to make the day and night 20 hours instead of 24. Why not?

Centuries ago it might have been practicable to do this; but now that time-keepers have become universal we could not make such a re-division. We might get all the church-clocks altered, but people would refuse to replace their old watches by new ones.

I fancy conservatism will be too strong for you in another case—that of the compass. The divisions of this are, like many other sets of divisions, made by halving and re-halving and again halving, until 32 points are obtained. Is it that the habits of sailors are so fixed as to make hopeless the adoption of decimal divisions?

Another reason has prevented—the natural relations of the cardinal points. The intervals included between them are necessarily four right angles, and this precludes a division into tenths.

Exactly. Here, as before, Nature is against you. The quadrant results from space-relations which are un-

changeable, and necessarily impose, in this as in other cases, division into quarters. Nature's lead has been followed by mankind in various ways. Beyond the quarter of a year we have the moon's four quarters. The quarter of an hour is a familiar division, and so is the quarter of a mile. Then there are the quarter loaf, and the quarter of a hundredweight. Though the yard is divided into feet and inches, yet in every draper's shop yards are measured out in halves, quarters, eighths, and sixteenths or nails. Then we have a wine merchant's quarter-cask, we have the fourth of a gallon or quart, and, beyond that, we have for wine and beer, the quarter of a quart, or half-pint.

Even that does not end the quartering of measures, for at the bar of a tavern quarters of gin, that is quarter-pints of gin, are sold. Evidently we must have quarters. What do you do about them? Ten will not divide by four.

The Americans have quarter dollars.

And are inconsistent in having them. Just as in France, notwithstanding the metric system, they speak of a quarter of a litre, and a quarter of a livre, so in the United States, they divide the dollar into quarters, and in so doing depart from the professed mode of division in the very act of adopting it—depart in a double way. For the tenths of the dollar play but an inconspicuous part. They do not quote prices in dollars and dimes. I continually see books advertised at 25c., 75c., \$1.25c., \$1.75c., and so forth; but I do not see any advertised at \$1.3 dimes or 4 dimes, &c. So that while not practically using the division theoretically appointed, they use the division theoretically ignored.

It may be somewhat inconsistent, but there is no practical inconvenience.

I beg your pardon. If they had a 12-division of the dollar, instead of a 10-division, these prices \$1.25 and \$1.75 would be \$1.3 and \$1.9. And not only would

there be a saving in speech, writing, and printing, but there would be a saving in calculation. Only one column of figures would need adding up where now there are two to add up; and, besides decreased time and trouble, there would be fewer mistakes. But leaving this case of the dollar, let us pass to other cases. Are we in all weights, all measures of length, all areas and volumes, to have no quarters?

Quarters can always be marked as $\cdot 25$.

So that in our trading transactions of every kind we are to make this familiar quantity, a quarter, by taking two-tenths and five-hundredths! But now let me ask a further question—What about thirds? In our daily life division by three often occurs. Not uncommonly there are three persons to whom equal shares of property have to be given. Then in talk about wills of intestates one hears of widows' thirds; and in Acts of Parliament the two-thirds majority often figures. Occasionally a buyer will say,—“A half is more than I want and a quarter is not enough; I will take a third.” Frequently, too, of medicines, where half a grain is too much or not enough, one-third of a grain or two-thirds of a grain is ordered. Continually thirds are wanted. How do you arrange? Three threes do not make ten.

We cannot make a complete third.

You mean we must use a make-shift third, as a make-shift quarter is to be used?

No; unfortunately that cannot be done. We signify a third by $\cdot 3333$, &c.

That is to say, you make a third by taking 3 tenths, *plus* 3 hundredths, *plus* 3 thousandths, *plus* 3 ten-thousandths, and so on to infinity!

Doubtless the method is unsatisfactory, but we can do no better.

Nevertheless you really think it desirable to adopt universally for measurements of weight, length, area,

capacity, value, a system which gives us only a make-shift quarter and no exact third?

These inconveniences are merely set-offs against the great conveniences.

Set-offs you call them! To me it seems that the inconveniences outweigh the conveniences.

But surely you cannot deny those enormous evils entailed by our present mixed system, which the proposed change would exclude.

I demur to your assertion. I have shown you that the mixed system would in large part remain. You cannot get rid of the established divisions of the circle and the points of the compass. You cannot escape from those quarters which the order of Nature in several ways forces on us. You cannot change the divisions of the year and the day and the hour. It is impossible to avoid all these incongruities by your method, but there is another by which they may be avoided.

You astonish me. What else is possible?

I will tell you. We agree in condemning the existing arrangements under which our scheme of numeration and our modes of calculation based on it, proceed in one way, while our various measures of length, area, capacity, weight, value, proceed in other ways. Doubtless, the two methods of procedure should be unified; but how? You assume that, as a matter of course, the measure-system should be made to agree with the numeration-system; but it may be contended that, conversely, the numeration-system should be made to agree with the measure-system—with the dominant measure-system, I mean.

I do not see how that can be done.

Perhaps you will see if you join me in looking back upon the origins of these systems. Unable to count by giving a name to each additional unit, men fell into the habit of counting by groups of units and compound groups. Ten is a bundle of fingers, as you may still see

in the Roman numerals, where the joined fingers of one hand and the joined fingers of the two hands are symbolized. Then, above these, the numbering was continued by counting two tens, three tens, four tens, &c., or 20, 30, 40 as we call them, until ten bundless of ten had been reached. Proceeding similarly, these compound bundles of tens, called hundreds, were accumulated until there came a doubly-compound bundle of a thousand; and so on. Now, this process of counting by groups and compound groups, tied together by names, is equally practicable with other groups than 10. We may form our numerical system by taking a group of 12, then 12 groups of 12, then 12 of these compound groups; and so on as before. The 12-group has an enormous advantage over the 10-group. Ten is divisible only by 5 and 2. Twelve is divisible by 2, 3, 4, and 6. If the fifth in the one case and the sixth in the other be eliminated as of no great use, it remains that the one group has three times the divisibility of the other. Doubtless it is this great divisibility which has made men in such various cases fall into the habit of dividing into twelfths. For beyond the 12 divisions of the zodiac and the originally-associated twelve-month, and beyond the twelfths of the day, and beyond those fourths—sub-multiples of 12—which in sundry cases Nature insists upon, and which in so many cases are adopted in trade, we have 12 ounces to the pound troy, 12 inches to a foot, 12 lines to the inch, 12 sacks to the last; and of multiples of 12 we have 24 grains to the pennyweight, 24 sheets to the quire. Moreover, large sales of small articles are habitually made by the gross (12 times 12) and great gross ($12 \times 12 \times 12$). Again, we have made our multiplication table go up to 12 times 12, and we habitually talk of dozens. Now, though these particular 12-divisions are undesirable, as being most of them arbitrary and unrelated to one another, yet the facts make it clear that a general system of twelfths is called

for by trading needs and industrial needs; and such a system might claim something like universality, since it would fall into harmony with those natural divisions of twelfths and fourths which the metric system necessarily leaves outside as incongruities.

But what about the immense facilities which the method of decimal calculation gives us? You seem ready to sacrifice all these?

Not in the least. It needs only a small alteration in our method of numbering to make calculation by groups of 12 exactly similar to calculation by groups of 10; yielding just the same facilities as those now supposed to belong only to decimals. This seems a surprising statement; but I leave you to think about it, and if you cannot make out how it may be I will explain presently.

III.—The promised explanation may most conveniently be given by reproducing, with various alterations and additions, a letter I wrote about the matter last November twelvemonth to a distinguished man of science. Omitting the name, the letter ran thus:—

“The enclosed memoranda concerning advantages to be derived from the use of 12 as a fundamental number, were written more than 50 years ago, and have since been lying unused among my papers.

“I send them to you because you have lately been expressing a strong opinion in favour of the metric system, and of course your opinion will weigh heavily. From the days when the accompanying memoranda were set down, I have never ceased to regret the spreading adoption of a system which has such great defects, and I hold that its universal adoption would be an immense disaster.

“Of course I do not call in question the great advantages to be derived from the ability to carry the method of decimal calculation into quantities and values, and of course I do not call in question the desirableness of having some

rationally-originated unit from which all measures of lengths, weights, forces, &c., shall be derived. That, as promising to end the present chaos, the metric system has merits, goes without saying. But I object to it on the ground that it is inconvenient for various purposes of daily life, and that the conveniences it achieves may be achieved without entailing any inconveniences.

“One single fact should suffice to give us pause. This fact is that, notwithstanding the existence of the decimal notation, men have in so many cases fallen into systems of division at variance with it, and especially duodecimal division. Numeration by tens and multiples of ten has prevailed among civilized races from early times. What, then, has made them desert this mode of numeration in their tables of weights, measures, and values? They cannot have done this without a strong reason. The strong reason is conspicuous—the need for easy division into aliquot parts. For a long period they were hindered in regularizing their weights and measures by the circumstance that these had been derived from organic bodies and organic lengths—the carat and grain, for instance, or the cubit, foot, and digit. Organic weights and lengths thus derived were not definite multiples one of another, and where they were approximate multiples the numbers of these were irregular—would not conform to any system. But there early began, as among the Chaldeans, arrangements for bringing these natural measures into commensurable relations. By sexagesimal division (60 being the first number divisible both by 10 and 12) the Babylonian cubit was brought into relation with the Babylonian foot. The stages of change from nation to nation and from age to age, cannot, of course, be traced; but it suffices to recognize the fact that the tendency has been towards systems of easily-divisible quantities—the avoirdupois pound of 16 ounces, for instance, which is divisible into halves, into quarters, into eighths. But,

above all, men have gravitated towards a 12-division, because 12 is more divisible into aliquot parts than any other number—halves, quarters, thirds, sixths; and their reason for having in so many cases adopted the duodecimal division, is that this divisibility has greatly facilitated their transactions. When counting by twelves instead of by tens, they have been in far fewer cases troubled by fragmentary numbers. There has been an economy of time and mental effort. These practical advantages are of greater importance than the advantages of theoretical completeness. Thus, even were there no means of combining the benefits achieved by a method like that of decimals with the benefits achieved by duodecimal division, it would still be a question whether the benefits of the one with its evils were or were not to be preferred to the benefits of the other with its evils—a question to be carefully considered before making any change.

“But now the important fact, at present ignored, and to which I draw your attention, is that it is perfectly possible to have all the facilities which a method of notation like that of decimals gives, along with all the facilities which duodecimal division gives. It needs only to introduce two additional digits for 10 and 11 to unite the advantages of both systems. The methods of calculation which now go along with the decimal system of numeration would be equally available were 12 made the basic number instead of 10. In consequence of the association of ideas established in them in early days and perpetually repeated throughout life, nearly all people suppose that there is something natural in a method of calculation by tens and compoundings of tens. But I need hardly say that this current notion is utterly baseless. The existing system has resulted from the fact that we have five fingers on each hand. If we had had six on each there would never have been any trouble. No man would ever have dreamt of numbering by tens, and the advantages of duo-

decimal division with a mode of calculation like that of decimals, would have come as a matter of course.

“Even while writing I am still more struck with the way in which predominant needs have affected our usages. Take our coinage as an example. Beginning at the bottom we have the farthing ($\frac{1}{4}$ penny), the halfpenny and penny (or one-twelfth of a shilling); next we have the threepenny piece ($\frac{1}{4}$ shilling), the *6d.* piece ($\frac{1}{2}$ shilling), and the shilling; and then above them we have the eighth of a pound (*2s. 6d.*), the quarter of a pound (*5s.*), and half-pound (*10s.*). That is to say, daily usage has made us gravitate into a system of doubling and again doubling and re-doubling; and when, until recently, there existed the *4d.* piece, we had the convenience of a third as well as a half and a quarter—a convenience which would have been retained but for the likeness of the *3d.* and *4d.* coins. And observe that this system of multiples and sub-multiples has its most conspicuous illustration in the commonest of all processes—retail payments—and that, too, in the usages of a nation which is above all others mercantile.

[Since this letter was written I have been struck by the fact that the ancient wise men of the East and the modern working men of the West, have agreed upon the importance of great divisibility in numerical groups. The Chaldean priests, to whom we owe so much, doubtless swayed in part by their astronomical arrangements, adopted the sexagesimal system of numeration, which at the same time facilitates in a special manner the division into aliquot parts. For 60 may be divided by ten different numbers—2, 3, 4, 5, 6, 10, 12, 15, 20, 30. From this significant fact turn now to the fact presented in our ordinary foot-rule. Each of its 12 inches is halved and re-halved, giving halves, quarters, and eighths. And then, if we consider the sub-divided foot as a whole, it gives us ten sets of aliquot parts. Beyond its 12ths the divi-

sions yield $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{6}$, $\frac{1}{8}$ ($1\frac{1}{2}$ inch), $\frac{1}{16}$ ($\frac{3}{4}$ inch), $\frac{1}{24}$ ($\frac{1}{2}$ inch), $\frac{1}{32}$ ($\frac{3}{8}$ inch), and $\frac{1}{48}$ ($\frac{1}{4}$ inch). And this ordinary mode of dividing the foot-rule results from the experience of centuries; for builders, carpenters, and mechanics, always buying footrules which best serve their needs, have gradually established the most useful set of divisions. And yet, though the early men of science and the modern men of practice are at one in recognizing the importance of great divisibility, it is proposed to establish a form of measure characterized by relative indivisibility!]

“Now it seems to me that the two facts—first, that in early days men diverged from the decimal division into modes of division which furnished convenient aliquot parts, and second, that where, as in America, the decimal system has been adopted for coinage, they have in the focus of business fallen into the use of aliquot parts in spite of the tacit governmental dictation—not only prove the need for this mode of division, but imply that, if the metric system were universally established, it would be everywhere traversed by other systems. To ignore this need, and to ignore the consequences of disregarding it, is surely unwise. Inevitably the result must be a prevention of the desired unity of method: there will be perpetual inconveniences from the conflict of two irreconcilable systems. [At the time this prophecy was made, I did not know that in California the “long bits” and “short bits” of the dollar, already illustrated this conflict of systems and its evils.]

“I fully recognize the difficulties that stand in the way of making such changes as those indicated—difficulties greater than those implied by the changes which adoption of the metric system involves. The two have in common to overcome the resistance to altering our tables of weights, measures, and values; and they both have the inconvenience that all distances, quantities, and values, named in records of the past, must be differently

expressed. But there would be further obstacles in the way of a 12-notation system. To prevent confusion different names and different symbols would be needed for the digits, and to acquire familiarity with these, and with the resulting multiplication-table would, of course, be troublesome: perhaps not more troublesome, however, than learning the present system of numeration and calculation as carried on in another language. There would also be the serious evil that, throughout all historical statements, the dates would have to be differently expressed; though this inconvenience, so long as it lasted, would be without difficulty met by enclosing in parenthesis in each case the equivalent number in the old notation. But, admitting all this, it may still be reasonably held that it would be a great misfortune were there established for all peoples and for all time a very imperfect system, when with a little more trouble a perfect system might be established."

Thus far the letter. And now let me sum up the evidence. Professedly aiming to introduce uniformity of method, the metric system cannot be brought into harmony with certain unalterable divisions of space nor with certain natural divisions of time, nor with the artificial divisions of time which all civilized men have adopted. As 10 is divisible only by 5 and 2 (of which the resulting fifth is useless), its divisibility is of the smallest; and having only a makeshift fourth and no exact third, it will not lend itself to that division into aliquot parts so needful for the purposes of daily life. From this indivisibility it has resulted that, though men from the beginning had in their ten fingers the decimal system ready made, they have, in proportion as civilization has progressed, adopted, for purposes of measurement and exchange, easily divisible groups of units; and in a recent case, where the 10-division of money has been imposed upon them, they have, under pressure of business needs,

abandoned it for the system of division into halves, quarters, eighths, sixteenths. On the other hand, the number 12 is unique in its divisibility—yields two classes of aliquot parts; and for this reason has been in so many cases adopted for weights, measures, and values. At the same time it harmonizes with those chief divisions of time which Nature has imposed upon us and with the artificial divisions of time by which men have supplemented them; while its sub-multiple, 4, harmonizes with certain unalterable divisions of space, and with those divisions into quarters which men use in so many cases. Meanwhile, if two new digits for 10 and 11 be used, there arises a system of calculation perfectly parallel to the system known as decimals, and yielding just the same facilities for computation—sometimes, indeed, greater facilities, for, as shown in the memoranda named in the above letter, it is even better for certain arithmetical processes.

Do I think this system will be adopted? Certainly not at present—certainly not for generations. In our days the mass of people, educated as well uneducated, think only of immediate results: their imaginations of remote consequences are too shadowy to influence their acts. Little effect will be produced upon them by showing that, if the metric system should be established universally, myriads of transactions every day will for untold thousands of years be impeded by a very imperfect system. But it is, I think, not an unreasonable belief that further intellectual progress may bring the conviction that since a better system would facilitate both the thoughts and actions of men, and in so far diminish the friction of life throughout the future, the task of establishing it should be undertaken.

Hence I contend that adoption of the metric system, while it would entail a long period of trouble and confusion, would increase the obstacles to the adoption of a perfect system—perhaps even rendering them insuper-

able—and that, therefore, it will be far better to submit for a time to the evils which our present mixed system entails.

P.S.—A mathematician and astronomer, who writes—“I am much interested in your letters and agree with almost everything,” makes some comments. He says:—“It has always been an astonishing thing to me that the advocates of decimalization do not perceive that its only advantage is in computation. In every other process it is a detriment.” Concerning the 12-notation, he remarks that “the advantages are notorious to all mathematicians.” Apparently less impressed than I am with the advance of knowledge from uncivilized times to our own and the breaking down of habits, now going on with accelerating rapidity, he does not share the expectation that the 12-notation “will ever be adopted in practice”: the obstacles to the change being too great. But without opposing the metric system, as threatening to stand in the way of a more perfect system, he opposes it as intrinsically undesirable, saying:—“I think that all that can be done is to make our coinage and measures as little decimal as possible, and our computation as decimal as may be.”

IV.—From one who every month has to act as auditor, I have received a letter in which he says:—“I had to go over more than £20,000 of accounts yesterday and was very thankful that it was not in francs.”

This statement, coming from a man of business, has suggested to me the question—By whose advice is it that the metric system of weights, measures, and values is to be adopted? Is it by the advice of those who spend their lives in weighing and measuring and receiving payments for goods? Is it that the men who alone are concerned in portioning out commodities of one or other kind to customers and who have every minute need for using this or that division or sub-division of weights or measures,

have demanded to use the decimal system? Far from it. I venture to say that in no case has the retail trader been consulted. There lies before me an imposing list of the countries that have followed the lead of France. It is headed "Progress of the Metric System." It might fitly have been headed "Progress of Bureaucratic Coercion." When fifty years after its nominal establishment in France, the metric system was made compulsory it was not because those who had to measure out commodities over the counter wished to use it but because the Government commanded them to do so; and when it was adopted in Germany under the Bismarckian *régime*, we may be sure that the opinions of shopkeepers were not asked. Similarly elsewhere, its adoption has resulted from the official will and not from the popular will.

Why has this happened? For an answer we must go back to the time of the French Revolution, when scientific men were entrusted with the task of forming a rational system of weights, measures, and values for universal use. The idea was a great one, and, allowing for the fundamental defect on which I have been insisting, it was admirably carried out. As this defect does not diminish its great convenience for scientific purposes the system has been gradually adopted by scientific men all over the world: the great advantage being that measurements registered by a scientific man of one nation are without any trouble made intelligible to men of other nations. Evidently moved by the desire for human welfare at large, scientific men have been of late years urging that the metric system should be made universal, in the belief that immense advantages, like those which they themselves find, will be found by all who are engaged in trade. Here comes in the error. They have identified two quite different requirements. For what purpose does the man of science use the metric system? For processes of measurement. For what purpose is the trader to use

it? For processes of measurement *plus* processes of exchange. This additional element alters the problem essentially. It matters not to a chemist whether the volumes he specifies in cubic-centimètres or the weights he gives in grammes, are or are not easily divisible with exactness. Whether the quantities of liquids or gases which the physicist states in litres can or cannot be readily divided into aliquot parts is indifferent. And to the morphologist or microscopist who writes down dimensions in sub-divisions of the mètre, the easy divisibility of the lengths he states is utterly irrelevant. But it is far otherwise with the man who all day long has to portion out commodities to customers and receive money in return. To satisfy the various wants of those multitudes whose purchases are in small quantities, he needs measures that fall into easy divisions and a coinage which facilitates calculation and the giving of change. Force him to do his business in tenths and he will inevitably be impeded.

“But you forget that the metric system is approved by many mercantile men and that its adoption is urged by Chambers of Commerce.” No, I have not forgotten; and if I had I should have been reminded of the fact by the fears now expressed that our commerce will suffer if we do not follow in the steps of sundry other nations. The fears are absurd. French and German merchants, when sending goods to England, find no difficulty in marking them or invoicing them in English measures. And if English merchants imply that they are too stupid to follow the example in a converse way, they can scarcely expect to be believed. Surely the manufacturers who supply them with piece-goods will make these up in so many mètres instead of in so many yards if asked to do so; and similarly in all cases. Or if not, it needs but a table on the wall in the clerks’ office, giving in parallel columns the equivalents of quantity in English denomi-

nations and French denominations, to make easy the needful invoicing and labelling. But it is not on this flimsiest of reasons that I wish chiefly to comment. The fact here to be specially emphasized is that merchants are not in the least concerned with the chief uses of the metric system. Their bales and chests and casks contain large quantities—dozens of yards, hundredweights, gallons. They do not deal with sub-divisions of these. Whether the retailer is or is not facilitated in portioning out these large quantities into small quantities is a question having no business interest for them. More than this is true. Not only have they never in their lives measured out fractional amounts in return for small sums of money, but they have rarely witnessed the process. Their domestic supplies are obtained by deputy, usually in considerable quantities; and neither behind the counter nor before it have they with any frequency seen the need for easy divisibility into aliquot parts. Their testimony is supposed to be that of practical men, while in respect of the essential issue—the use of weights and measures for retail trade—they have had no practice whatever.

See then the strange position. The vast majority of our population consists of working people, people of narrow incomes, and the minor shopkeepers who minister to their wants. And these wants daily lead to myriads of purchases of small quantities for small sums, involving fractional divisions of measures and money—measuring transactions probably fifty times as numerous as those of the men of science and the wholesale traders put together. These two small classes, however, unfamiliar with retail buying and selling, have decided that they will be better carried on by the metric system than by the existing system. Those who have no experimental knowledge of the matter propose to regulate those who have! The methods followed by the experienced are to be re-arranged by the inexperienced!

Intentionally or unintentionally those who have bad cases to defend very commonly raise false issues. It has been so in this case. Such responses as I have seen to the foregoing arguments have assumed or asserted that I uphold our existing system of weights, measures, and moneys; and they assert this because I have pointed to various conveniences which these have. But if this ascription does not result from a wilful misrepresentation, it results from an unintelligent attention to the argument. The chaotic character of our modes of specifying quantities is as manifest to me as to the metricists. When instancing as convenient these or those tables now in use, I have referred to the *mode of division*; not at all intending to imply approval of the particular sizes or amounts of the divisions: these being in many cases very undesirable.

All who do not perversely misinterpret must surely recognize my thesis as having been that, rather than establish a fundamentally imperfect system based upon 10 as a radix, it will be better to wait until we can change our system of numeration into one with 12 as a radix; and then on that to base our system of weights, measures, and values: tolerating present inconveniences as well as we may. Opponents do not deny that a 12-system of numeration would be better than is the 10-system, and do not deny that weights, measures, and values would be more conveniently expressed in terms of a 12-system. Their contention is that the change to a 12-system of numera-

tion is not practicable. Tacitly they assume that because people are not now sufficiently intelligent to perceive its advantages, and to take the trouble of making the needful changes, they never will be sufficiently intelligent.

It is strange that with past experiences before them their imaginations should thus fail them. See what lessons history reads us. If our cannibal ancestors, who in the forests of Northern Europe two thousand or more years ago sheltered in wigwams and clothed themselves in skins, had been told that some of their descendants would live in massive towers of stone and cover their bodies with metal plates, explanations, even could they have been understood, would have left them utterly incredulous. Or, again, if the mediæval barons had been told that in a few centuries after their deaths, nobles, instead of needing castles and armour, would live in houses which even a solitary thief could break into, and would walk about unarmed without attendants, they would have thought their informant insane. Yet with such cases before them, cultivated classes in our own day suppose that future usages will be like present ones, and that the culture, ideas, and sentiments now prevailing will always prevail; and they suppose this though men's feelings and thoughts have become more plastic than they ever were before. They cannot conceive that hereafter people may think it worth while to make a revolution (not much more troublesome than that which they advocate) for the purpose of greatly facilitating the billions of transactions,

commercial, industrial, and other, daily gone through by mankind.

If, as seems probable, they should have their way—if the Act of Parliament just passed, giving permission to use the Metric System, should presently be followed, as they intend it to be, by an Act making the use of the Metric System compulsory—if in the United States as well as in England and its colonies, governments prompted by bureaucracies, but not consulting the people and clearly against their wishes, should make universal this gravely defective system, very possibly it will remain thereafter unalterable. When the trade within each nation as well as all international commerce has been unified in method, the obstacles to a radical change may be insuperable; even though most should come to see the great superiority of another method. And should this happen, then men of the future looking back on men of the present, will say of them that, having before them a system which they recognized as relatively perfect, they deliberately imposed a relatively imperfect system on all mankind for all time.

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EXTRACT FROM THE PROVISIONAL PREFACE.

Something to introduce the work of which an instalment is annexed, seems needful, in anticipation of the time when completion of a volume will give occasion for a Permanent Preface.

In preparation for *The Principles of Sociology*, requiring as bases of induction large accumulations of data, fitly arranged for comparison, I, some twelve years ago, commenced, by proxy, the collection and organization of facts presented by societies of different types, past and present; being fortunate enough to secure the services of gentlemen competent to carry on the process in the way I wished. Though this classified compilation of materials was entered upon solely to facilitate my own work; yet, after having brought the mode of classification to a satisfactory form, and after having had some of the Tables filled up, I decided to have the undertaking executed with a view to publication; the facts collected and arranged for easy reference and convenient study of their relations, being so presented, apart from hypothesis, as to aid all students of Social Science in testing such conclusions as they have drawn and in drawing others.

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